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The Society traces its origin to the *Philosophical Society of Australasia* founded in Sydney in 1821. The Society exists for "*the encouragement of studies and investigations in Science Art Literature and Philosophy*": publishing results of scientific investigations in its *Journal and Proceedings*; conducting monthly meetings; awarding prizes and medals; and by liaising with other learned societies within Australia and internationally. Membership is open to any person whose application is acceptable to the Society. Subscriptions for the Journal are also accepted. The Society welcomes, from members and non-members, manuscripts of research and review articles in all branches of science, art, literature and philosophy for publication in the *Journal and Proceedings*.

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Editorial

Robert Marks

Economics, University of New South Wales, Sydney

E-mail: robert.marks@gmail.com

Who is the only New South Wales-born Nobel laureate so far? The chemist, Sir John "Kappa" Cornforth AC FRS (1917–2013).[1] A graduate of Sydney University, he and his wife, Rita née Harradence, another chemist, independently won 1851 Exhibitions to Oxford in the late 'thirties. Their first publications were in this *Journal* some eighty years ago (Harradence & Lions 1936, Cornforth et al. 1937). Cornforth's 1975 Nobel was awarded "for his work on the stereochemistry of enzyme-catalyzed reactions."[2]

Thinking about some way for the Society to remember John Cornforth,[3] I wondered whether we could perhaps publish his last piece. I came upon reference to the talk, "The Hidden Asymmetry of Life — why the hidden asymmetry of most living things is fundamental to life and how it is manifested," given in Canberra at the Australian Academy of Science on 9 November 1977. Sounded like an interesting piece, and not too technical, so I tried to find the text. Unsuccessfully. Eventually, at the Royal Society in London I came across an unpublished address, "Adventures with Sugars," delivered at the University of Sussex on 23 July 1999. With the Royal Society of London's permission and with John Cornforth's family's permission, this address is here published for the first time. Its publication is a suitable bookend to John Cornforth's publishing career: with an eighty-year span, his first and last publications appear here, in the *Journal & Proceedings of the Royal Society of New South Wales*.[4]

Recently a friend rang me and told me about an article in *The Monthly* of October, 2018. I bought the issue, read the article she had mentioned (on drug policy), and then noticed another, about one of the founders, two hundred years ago, of the Philosophical Society of Australasia, Judge Barron Field (1786–1846).[5] When constructing the on-line archive of papers presented to the Society and printed in the *Journal & Proceedings*, I had come across an 1825 book, published in London, and edited by Barron Field, his *Geographical Memoirs on New South Wales*, which contained the earliest records of papers presented in 1821 at meetings of the Philosophical Society. I added the book's chapters to the on-line archive of the *Journal & Proceedings*.[6] *The Monthly* article was on Field and his poems, reprinted at the back of his 1825 book.[7]

[1] Patrick White (1912–1990) was born in England of Australian parents.

[2] See https://www.nobelprize.org/search/?s=Cornforth

[3] Past President Don Hector had approached Professor Cornforth to join the Society as an FRSN, but he declined: age and distance.

[4] See https://royalsoc.org.au/council-members-section/234-cornforth

[5] See http://adb.anu.edu.au/biography/field-barron-2041

[6] https://royalsoc.org.au/links-to-papers- since-1856

[7] See http://gutenberg.net.au/ebooks13/1304421h.html#ch22

Two thoughts occurred to me: the approaching two-hundredth anniversary of the Society, and our recent determination to widen to the ambit of the Society, to encompass "Science, Art, Literature and Philosophy." I contacted the author, Professor Justin Clemens at Melbourne University, and *The Monthly*, and permission to reprint was quickly received. The *Journal* has never published a paper about Barron Field, and Clemens' paper is much more than a review of his poetry, giving a richer idea of Field's time in Sydney, including his role in coining the phrase *terra nullius*. Clemens is the erstwhile art critic for *The Monthly*.

This issue includes three submitted papers. The first is by Dragovic and Bajpai, on the issue of estimating erosion on paths in the Royal National Park. This study continues our publication of works on the Australian environment.

A year ago we published a report from 1885 by the Rev. Julian Tenison-Woods (2017) on the geology of Malacca written for the colonial government of the Straits Settlements. Tenison-Woods was a frequent contributor to the *Journal* in the late nineteenth century.[8] Roderick O'Brien, who uncovered the earlier report, has for this issue found an address given by Tenison-Woods in Hong Kong about the mines and minerals of the Malay Peninsula, published in a local newspaper of the time, on 3 February 1885.

As well as a discourse about Malayan geology, the piece is of interest by contrasting European technology against Chinese contracting in mining tin in Malaya. The industrial revolution had equipped European miners with new technology — machinery, explosives, mechanised transport — but Tenison-Woods argues that, for mines both in Victoria[9] and in Malaya, the "tribune" system of contracting had produced higher profits. As we would say, the tribune system of contracting shifted the risk from the mine owners to the contracting labourers: instead of paying the miners for the amount of material they brought up, the tribune system paid the miners for the amount of tin they extracted. The incentives faced by the miners had changed, as in the Clunes mine, and the mines' lives would be extended and the owners' profitability enhanced, if not the miners'.

In 1974 I was a graduate student at Stanford, living in a studio apartment off-campus. In the same building I got to know Wendy Bracewell, daughter of Professor Ron Bracewell. I did not take any classes from her father, but I knew of him: the Australian radioastronomer, at a university where there were few if any Australian professors. Christmas was looming and Wendy was wondering what to give her father: "He's interested in trees," she told me (see Bracewell 2005). I suggested a recent book that I had just bought, and so Ron Bracewell was given a copy of Anthony Huxley's *Plant and Planet*, by the son of Julian Huxley FRS and the nephew of Aldous Huxley, author of *Brave New World*.

Irene Kelly, widow of past President Jak Kelly, tells me that the Kellys and the Bracewells were fast friends; she had visited him in Palo Alto just before his death.[10] On

[8] See the list in O'Brien (2017).

[9] Thompson, H.A. (1858), Description of the Clunes Gold Mine, Victoria, *The Sydney Magazine of Science and Art* 2: 79-80, 1859. (Paper presented at the Philosophical Society of N.S.W. on Aug. 11, 1858.) https://archive.org/stream/sydneymagazines01socigoog#page/n95/mode/1up

[10] At the 1269th OGM of 5 December 2018, in her acknowledgment of Anita Petzler, the 2018 Jak Kelly Award winner, Irene Kelly mentioned a colleague of Ron Bracewell's: pioneer radioastronomer, Ruby Payne-Scott; see Halleck (2018).

16 May 1978, Ron Bracewell gave the Society's Pollock Memorial Lecture, "Life in outer space" (Bracewell 1979). Ron Bracewell died in 2007, weeks after his 86th birthday, and I decided to include a piece on his life. With permission, we are reprinting an obituary by Thompson and Frater, first published in 2010. We are also publishing the edited transcript of a previously unpublished interview between Ron Bracewell and Ragbir Bhathal FRSN, recorded in 2000 in Sydney. A recent book puts Bracewell's pioneering work into context: Frater, Goss, and Wendt (2017); he should have been FRS, as so many of his collaborators in Australia and abroad were. (Indeed, given his mathematics, some have argued that he should have shared the 1979 Nobel Prize in Physiology or Medicine with Cormack and Hounsfield, "for the development of computer assisted tomography.")[11]

Apart from our interest in trees, another point of connection between Bracewell and me, it turns out, is Ron Bracewell's development of the Hartley Transform, named after Ralph Hartley (1888–1970), a mathematician and engineer at Bell Labs. Recent work of mine has also used research of Ralph Hartley's: his Hartley (1928) measure of information (a necessary forerunner to Shannon's 1948 work) solved the problem of how to measure the amount of uncertainty associated with a finite set of possible alternatives. I use the Generalised Hartley Measure to measure the distance between pairs of sets of vectors in work exploring validation of computer simulations of real-world phenomena (Marks 2019).

David Hush FRSN is an eminent composer. He offered to write a piece for the *Journal*, and after some discussion we decided on Mozart's music. So his paper and Clemens' paper on Barron Field, Australia's first published poet, move the *Journal* away from its previous focus on hard science, towards the wider remit of "Science, Art, Literature and Philosophy."

Earlier this year Len Fisher FRSN was selected as a finalist in the Stockholm-based "New Shape" competition to suggest new institutional ways to deal with global challenges; his entry was one of 14 chosen from an initial field of 2,702 entries. Len agreed to my suggestion that he write an account of the Competition, his analysis of global catastrophic risks facing mankind,[12] and the governance solution he had proposed in the Competition. His write-up of this is published below, including his thoughts about his lack of success at winning the US$5 million prize.

When I took over the editorship, I talked with Michael Burton, the previous editor, about the issue of selecting excellent theses for the PhD abstracts section. We agreed that the editor should ask the universities to choose a few excellent theses. Easier said than done: the newer, smaller universities have been good at responding to my enquiries, but the larger ones (Sydney, UNSW, ANU, Macquarie, Wollongong) have been quite dilatory. This time around I thought I'd use the results of the Three-Minute Thesis (3MT) competitions to identify good work. Problem is that students usually don't complete for some time (years) after the 3MT. Nonetheless, two of the four Abstracts derive from the 3MT: Dr. Kaye-Smith's and Dr. Marks'.[13]

[11] In 1992 he was elected to foreign associate membership of the Institute of Medicine of the US National Academy of Sciences, the first Australian to achieve that distinction, for fundamental contributions to medical imaging.

[12] See U.S. Global Change (2018).

[13] Full disclosure: Dr. Marks is my daughter; she was the Monash runner-up in the 3MT in 2015.

In future, I'd like the universities to be more forthcoming with excellent theses.

A note arising from the address by Tom Keneally DistFRSN in the June 2018 issue: he has recently published the historical novel he spoke of in May, tying Mungo Man with today: *Two Old Men Dying* (Keneally 2018).

Finally, I'd like to thank the team who helped me in finalising this issue: Ed Hibbert, Rory McGuire, and Jason Antony, as well as Eryl Brady, many reviewers and those who acted as sounding boards.

Balmain, 6 December 2018.

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Adventures with sugars

John Warcup Cornforth*

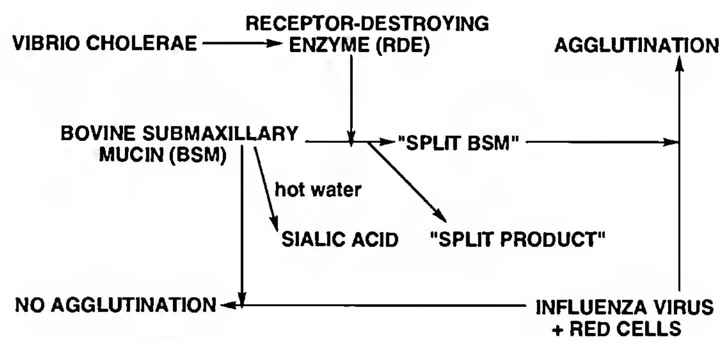
*The late Sir John "Kappa" Cornforth, FRS, is the only NSW-born Nobel Laureate.

Abstract

This is a presentation given at the Alan Johnson Memorial Lecture on 23 July 1999 at the School of Chemistry, University of Sussex. It is the last publication of Sir John Cornforth's (1917–2013) [http://oa.anu.edu.au/obituary/cornforth-sir-john-warcup-17375]. It has not previously been published (the transcript has been lightly edited by Robert Marks).

Like the rest of you, I am looking forward to the presentations of Professors A. G. Barrett and K. C. Nicolaou. They are very capably engaged in creating the present and future of organic chemistry. My own present and future are limited, but I have a rather extensive past and I propose in my talk to revisit one aspect of this. My excuse is that I have never lectured about it before. Those of you who are digesting a good lunch should feel free to snooze peacefully before the real business begins. But wake up by three o'clock — I know my duty as first speaker and I shall not overrun.

The title of this talk might suggest that I am a sugar[1] chemist. Nothing could be further from the truth. But 43 years ago I was a scientist at the National Institute for Medical Research at Mill Hill. Within the constraints of time and resources I, and my colleagues, were free to form whatever collaborations we chose, and my burden of accountability was discharged by a brief annual report to my Director. It happened about this time that the biochemist Alfred Gottschalk (1894–1973),[2] from Macfarlane Burnet's Walter and Eliza Hall Institute in Melbourne, was a visiting worker at Mill Hill. He was deeply interested in mucins, the slimy substances in saliva and in many other secretions. Because these contain amino-sugars he was interested in them too, and he endlessly discussed their chemistry with me and anyone else who would listen.



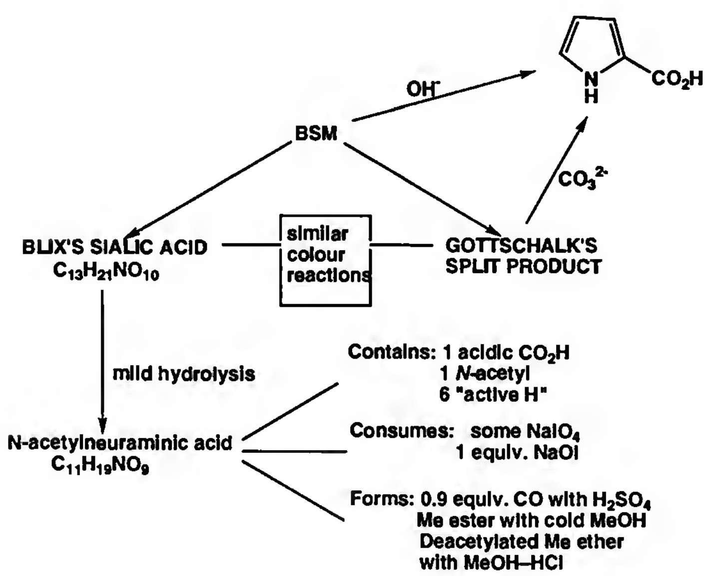
Macfarlane Burnet FRS (1899–1985)[3] had asked Gottschalk to study the interaction between mucins and the influenza virus (Figure 1). Some mucins were readily available, notably from the submaxillary glands of slaughtered cattle, and it had been found that these would inhibit the agglutination, by the influenza virus, of red blood cells. If the mucin had previously been treated with an enzyme preparation, then called receptor-destroying enzyme, from the cholera Vibrio, it lost this inhibitory

[1] As a chemical term, "sugar" usually refers to all carbohydrates of the general formula $C_n(H_2O)_n$.

[2] http://adb.anu.edu.au/biography/gottschalk-alfred-10336

[3] http://rsbm.royalsocietypublishing.org/content/roybiogmem/33/99

power and dialysable substance appeared in the medium. Gottschalk never got crystals from his preparation of this substance, but on the basis of three different colour reactions, he concluded that it was a sialic acid. He also made a significant observation about its chemistry. On heating in alkaline solution, bovine submaxillary mucin yielded pyrrole-2-carboxylic acid, and the same acid was formed from his "split product" in much milder alkali and in a yield consistent with its being the sole source of the acid from mucin (Figure 2).



The history of the sialic acids (the name is from the Greek word for saliva or spittle) began in 1936 when Gunnar Blix (1894–1981)[4] obtained a crystalline acid by heating bovine submaxillary mucin with water. Similar compounds were later isolated by Ernst Klenk (1869–1971)[5] from brain tissue and by Richard Kuhn (1900–1967)[6] from cow colostrum. They were all given different names and it took some time for this confusion to settle. Nowadays, sialic acid is the generic term and neuraminic acid (Klenk's name) is the parent molecule. The sialic acids are various acyl derivatives of neuraminic acid.

I must try to give you a picture of carbohydrate biochemistry as it was half a century ago. For sugar chemists, ion-exchange resins had taken a lot of drudgery out of the isolation of sugars. But one of the most liberating events had been the invention of paper chromatography. You could usually find a biochemistry department on a campus just by following your nose: you would be guided unerringly by the reek of isobutyric acid, which is a component of farmers' muck-heaps and of a useful mobile phase in sugar chromatography. And because the spots on the paper needed to be made visible, all kinds of more or less specific spray reagents came into use. Biochemists in those days relied much more on colour reagents both for detection and measurement, and the invention of reliable spectrophotometers was another liberating event. When I was a post-doctoral student, ultra-violet spectra had to be plotted from successive exposures on a glass photographic plate. The little Beckman DU spectrophotometer came like the gift of a Stradivarius to a virtuoso.

For chemists, infra-red spectroscopy was fairly well developed as a means of identification and detection of functional groups. X-ray crystallography was a sure way to identify a known compound but was slow and uncertain for structural elucidation. Mass spectrometry was still in its childhood and NMR newly born. On the other hand, organic chemists already had fairly satisfactory theories of structure and reactivity. As

[4] https://www.ncbi.nlm.nih.gov/pmc/articles/PMC4463483/

[5] https://www.encyclopedia.com/science/dictionaries-thesauruses-pictures-and-press-releases/klenk-ernst

[6] https://www.encyclopedia.com/people/science-and-technology/chemistry-biographies/richard-kuhn

a student of Robert Robinson (1886–1975)[7] I had been heavily exposed to these, whereas most biochemists had learned their chemistry as a separate subject taught by contemporaries or students of Emil Fischer (1852–1919).[8] Given a crystalline substance, their first care was elementary analysis. For Blix's sialic acid, this led eventually to an empirical formula $C_{13}H_{21}NO_{10}$. There was an O-acetyl group, very easily hydrolysed, and another acetyl group thought to be *N*-acetyl. Removal of the labile acetyl led to the compound now called *N*-acetylneuraminic acid, also available directly from sheep submaxillary mucin, for which the formula $C_{11}H_{19}NO_9$ was established. One obstacle in the way of this finding was the spontaneous formation of a methyl ester in dry methanol. The successful solvent for crystallization, found by Blix, was the strangest I ever heard of: methanol-water-ether-petrol. All four were essential.

Nowadays, the structure would be found out in a few days by crystallography or by pulsed NMR. As it was, this was one of the last natural products whose structure was divined from its chemistry alone. For a classic example from the same period, Robert B. Woodward's (1917–1979)[9] solution of the Terramycin structure[10] is supreme, but I reconstruct my own logical process here for its historical interest.

Little more of the chemistry of *N*-acetylneuraminic acid was known at the time. The substance was oxidizable by Willstätter's hypoiodite, and also by periodate since its spots on paper could be made visible by a treatment beloved of sugar chemists: periodate followed by Schiff's reagent. With hot methanolic HCl it gave a methyl ether with concomitant loss of the acetyl group. A now forgotten analytical procedure gave it six "active hydrogens:" that is, it liberated six equivalents of methane from methylmagnesium iodide. Discounting the carboxyl and acetamido hydrogens, that indicated four hydroxyls. But it also gave nearly one equivalent of carbon monoxide on warming with concentrated sulfuric acid.

Gottschalk prepared a note[11] to *Nature*, attempting to reconcile the data in terms of a partial structure. I forget the structure proposed; it was a pyrroline of some kind. But I saw the note before publication and I began to read the published work and to think independently. Blix's finding of carbon monoxide intrigued me. I knew that tertiary carboxylic acids readily yielded it on warming with sulfuric acid. But tertiary carboxylic acids are difficult to esterify; on the one hand, α-oxo acids form methyl esters very easily. Then the intellectual leap came: suppose that the substance was both!

It had sugar-like reactions: could it be a pyranose-carboxylic acid tautomeric, like sugars, with an open-chain α-oxo acid? This fitted many observations neatly. The formation of pyrrole-2-carboxylic acid placed the acetamido group on position 5 of the pyranose, excluding the furanose alternative, and it required an additional hydroxyl at the 4-position to provide, by dehydration, the second double bond. This left three carbons and three hydroxyls to be accounted for. Position 6 was the only place for them, and they had to be together in a chain because of

[7] https://www.encyclopedia.com/people/science-and-technology/chemistry-biographies/robert-robinson

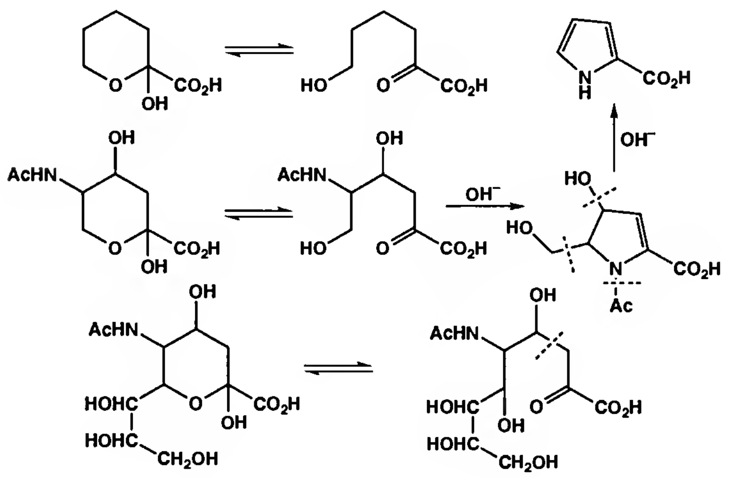
[8] https://www.encyclopedia.com/people/science-and-technology/chemistry-biographies/emil-fischer

[9] https://www.encyclopedia.com/people/science-and-technology/chemistry-biographies/robert-burns-woodward

[10] Stephens et al. (1952).

[11] Gottschalk (1954)

the periodate consumption. Suddenly I saw that I was looking at a potential aldol condensation product of an acetyl-hexosamine and pyruvic acid; both well-known natural products (Figure 3).



I wrote to Gottschalk suggesting this structure. He rewrote his note to adopt it, added a line at the end thanking me for helpful advice, and sent the note[12] to *Nature* without showing it to me. He also reported a new experiment: some pyrrole-2-carboxylic acid could be obtained by heating glucosamine with pyruvic acid in alkaline solution. When the note appeared, he explained that he too would have arrived at my structure in time.

I am less surprised now by Gottschalk's action than I was then. He had been studying this stuff for years and I was an interloper. And since he had published the structure without ever having seen an isolated specimen of the acid, I suppose that he in turn was an interloper to Blix, Klenk and Kuhn. But I am still surprised that Richard Kuhn, an excellent chemist who had won the Nobel Prize for Chemistry in 1938 for his work on carotenoids and who had all the information long before me, did not get the structure first.

Gottschalk and I had already agreed to collaborate on the synthesis of acetyl-neuraminic acid and I enlisted the help of Mary Daines[13] who was doing a London Ph.D. with me. We wanted to try the condensation of *N*-acetylhexosamine with pyruvic acid. Acetyl-glucosamine was the only one commercially available and it was prohibitively expensive. So the first step of the synthesis was to Scott's Restaurant in Piccadilly Circus, from which we obtained bucketsful of evil-smelling crab shells. We made several hundred grams of glucosamine hydrochloride from these, and put the *N*-acetyl group on most of it. Then we tried condensation with pyruvic acid in alkaline solution and this was unsuccessful.

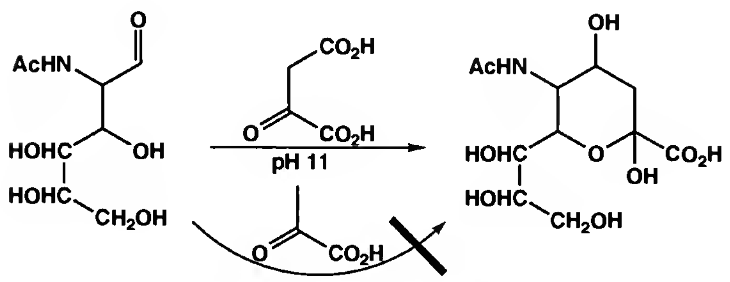
At this point I remembered a classic experiment by Robert Robinson.[14] He put together the alkaloid derivative tropinone in aqueous solution from methylamine, butanedial and acetone: a double Mannich condensation. But I also remembered what text-books omit: the yield of tropinone was so poor that he had had to isolate it as an insoluble derivative, whereas the condensation with acetonedicarboxylic acid instead of acetone yielded 40% of tropinone.

So we substituted oxaloacetic acid for pyruvic acid and tried again. After 2–3 days at pH 10–11, the solution showed promising colour reactions. We separated the acidic fraction and chromatographed it on activated carbon. This gave us fractions which were monitored by paper chromatography. Some of them crystallized, and the exotic solvent system of Blix then gave us pure *N*-acetylneuraminic acid, identical with natural material (Figure 4).

[12] Gottschalk (1955)

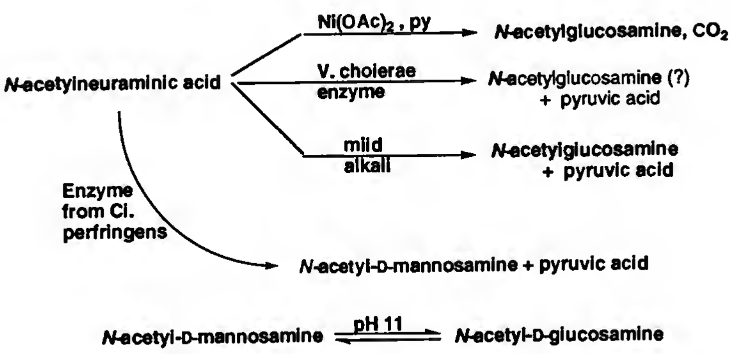
[13] Cornforth, Daines, Gottschalk (1957).

[14] Robinson (1917).

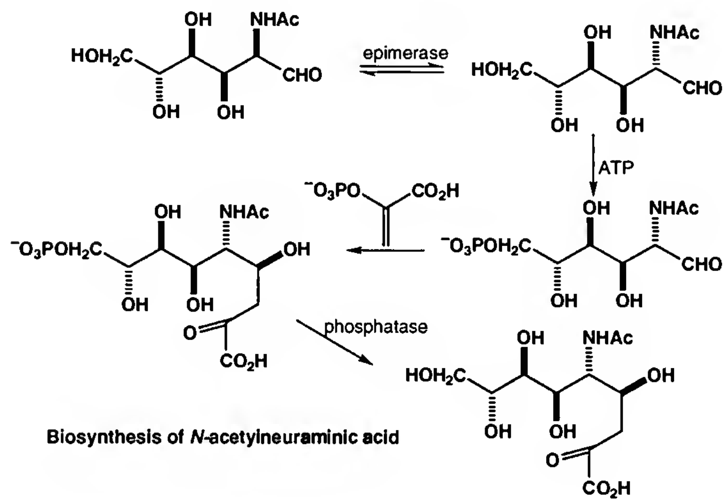


Synthesis of *N*-acetylneuraminic acid

Was this the end of the matter? No, not quite. During the work, Kuhn had reported that *N*-acetylneuraminic acid when heated in pyridine with nickel acetate yielded *N*-acetylglucosamine, and other workers showed that the acid is degraded to *N*-acetylglucosamine and pyruvic acid by mild alkali treatment. There was even a claim to have effected the same fission enzymically. But although *N*-acetylglucosamine was identified rigorously from the chemical fissions, identification with the enzymic fission was less secure; and Roseman and Comb[15] later showed that enzymic fission yields *N*-acetylmannosamine, which in alkali is equilibrated with *N*-acetylglucosamine. So our synthesis had depended on this equilibration occurring in the alkaline medium and its success contained a dollop of luck (Figure 5).



Actually, the synthesis imitates quite closely the biosynthesis, in which *N*-acetylglucosamine is epimerized to the mannosamine which, after phosphorylation at the 6-hydroxyl, is condensed with a different form of activated pyruvic acid: phosphoenol-pyruvate. The superfluous phosphate is then removed (Figure 6).



One can speculate that Nature uses this intermediate phosphorylation because it is impossible to obtain a good yield of the mannosamine from the glucosamine without disturbing the thermodynamic equilibrium by continuously removing the product. A phosphotransferase specific for *N*-acetylmannosamine does just that. Later Pat Carroll and I[16] found a preparative procedure for making *N*-acetylmannosamine from the glucosamine. We did it by selective crystallization and recycling, and we showed that *N*-acetylmannosamine yielded around five times more product on condensation with oxaloacetate. Even so, that was barely 10% and I regret having left this problem without devising a decent yield. As it is, the commercial supply of *N*-acetylneuraminic acid depends on a Chinese delicacy, bird's-nest soup. A species of sparrow[17] living on the Eastern shores of China makes small cup-shaped nests almost entirely from its own spit. These are the source of the soup, and thence of *N*-acetylneuraminic acid.

As time has passed, the sialic acids have become recognized as important compo-

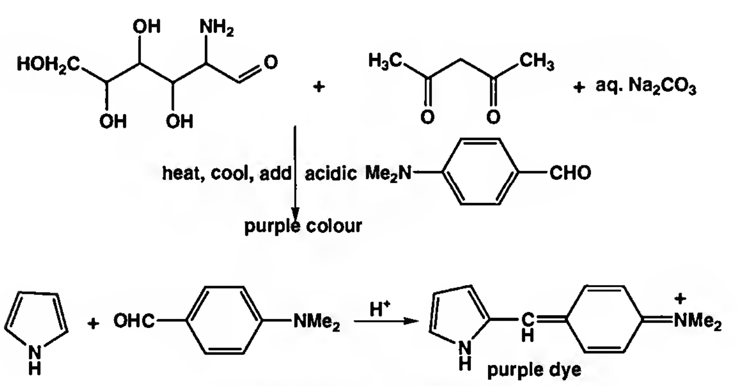
[15] Comb and Roseman (1960).

[16] Carroll and Cornforth (1960)

[17] The edible-nest swiftlet or white-nest swiftlet (*Aerodramus fuciphagus*).

nents of glycoproteins and glycolipids. The "receptor-destroying enzyme" is now called a neuraminidase and the influenza virus itself is known to produce a neuraminidase which it uses to liberate itself from infected cells and which is consequently a target in the chemotherapy of influenza.

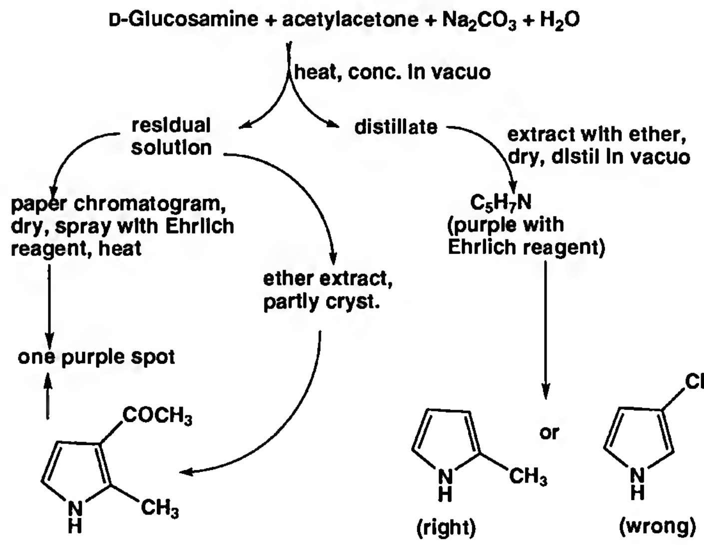
Gottschalk's interest in amino-sugars extended to their colour reactions. One of these is the Elson-Morgan reaction which was widely used for quantitative analysis of hexosamine in biological fluids and hydrolysates. Essentially, one heats the solution with acetylacetone and sodium carbonate at a pH around 9.8 and then adds the Ehrlich reagent, a solution of 4-dimethylaminobenaldehyde in ethanol and hydrochloric acid. A purple-red colour develops and in standard conditions its intensity is a measure of the hexosamine content. Gottschalk, a paper chromatography addict as many biochemists were at the time, took a rather concentrated reaction mixture from glucosamine and acetylacetone and chromatographed it on paper. He then dried the paper, sprayed it with the Ehrlich reagent, and heated it. A single purple spot developed. He found that ether extraction of the reaction mixture removed the precursor of this spot (Figure 7).



Ehrlich reaction with a pyrrole

I agreed to look at the chemistry of this finding, again with Mary Daines's help.[18]

We examined the literature on the Elson-Morgan reaction and found a 1951 paper which showed that at least one of the chromogens — that is, products generating the colour with Ehrlich's reagent — was volatile with steam, so we looked at this first. Gas-liquid chromatography at the time was in its infancy although its parents, Archer Martin and Tony James, were both working at Mill Hill; so we isolated the volatile component the hard way, by extraction of distillates with ether and final purification of the product by distilling it in vacuum at room temperature through a tube packed with magnesium perchlorate to dry it. It had the composition of a monomethylpyrrole (Figure 8).



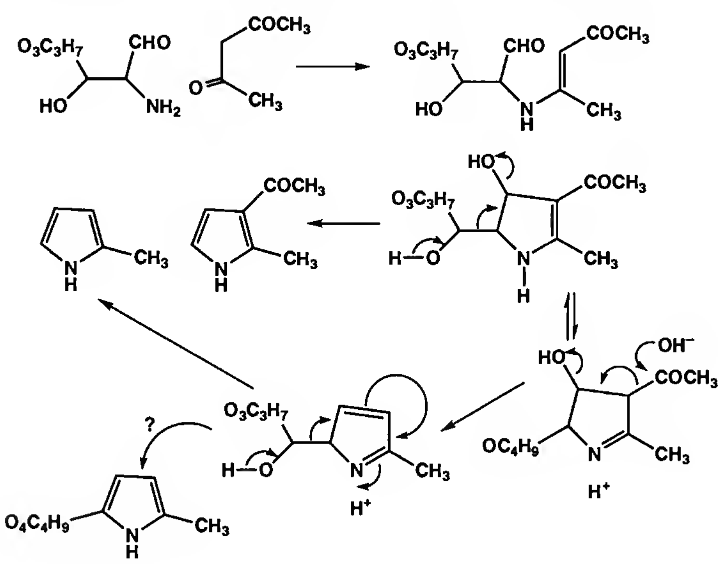
We had to make both 2- and 3-methylpyrrole synthetically in order to be sure which one it was, because virtually no physical data were available and there was a mix-up of the two pyrroles in Hans Fischer's standard monograph.[19] The description of the two pyrroles in this book has the distinction that every single fact in it is wrong. But the problem induced me to devise new syntheses of 3-methylpyrrole and the fact that Sigma can sell this pyrrole today is basically because Fischer got it wrong 65 years ago. We cleaned up other parts of this mess and that led to

[18] Cornforth and Firth (1958).

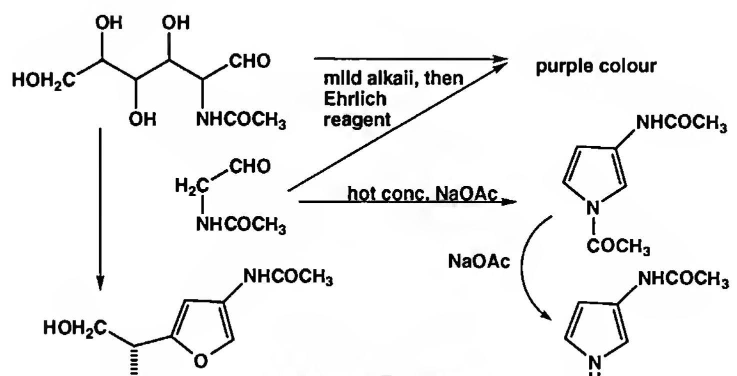
[19] Fischer and Orth (1940).

some interesting chemistry, but to recount it would take us too far from the sugars. For the glucosamine work it had negative value because the chromogen turned out to be identical with 2-methylpyrrole. The chemical yield of this pyrrole in the reaction with acetylacetone in analytical conditions is around 10%, and in the usual assay for hexosamine it is responsible for nearly 70% of the colour.

Gottschalk's single spot on paper chromatography turned out to be a red herring, or perhaps one should say a purple herring. We repeated the ether extraction of a concentrated reaction mixture and purified his crystalline product. It turned out, as he had suspected, to be 3-acetyl-2-methylpyrrole. It was a new compound and we verified its structure by a Knorr synthesis from aminoacetaldehyde and acetylacetone (Figure 9).



But the colour that it gave with the Ehrlich reagent in the conditions of analysis was so faint and so slow to develop that it could not have been responsible for as much as 1% of the colour in an Elson-Morgan assay (Fig 10). On the other hand, when it was spotted on paper and heated with the Ehrlich reagent it gave an intense colour. In Gottschalk's experiment he lost the main chromogen, 2-methylpyrrole, by evaporation when the paper was dried. When it was then heated after spraying with Ehrlich reagent, the hydrochloric acid in the reagent would have been concentrated sufficiently to effect acid-catalysed deacetylation, already known for other acetylpyrroles. The 2-methylpyrrole so formed would react fast with the colour-producing reagent, too fast to allow evaporation or polymerization. Thus, a minor actor had been promoted to the principal role.



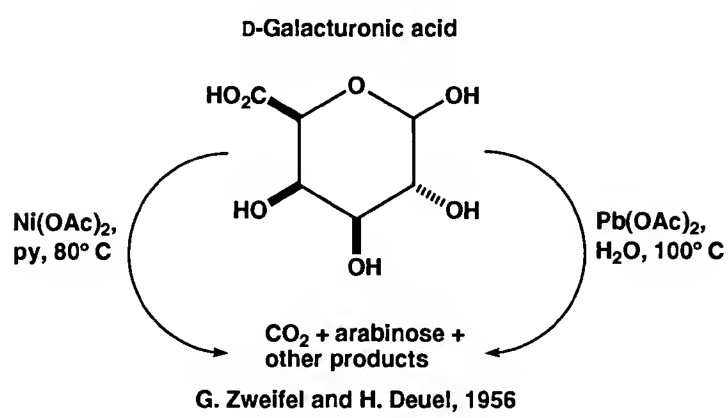
The Morgan-Elson assay.

The chemistry of chromogen formation is of some interest. In both 2-methylpyrrole and the 3-acetyl analogue, four of the six carbons in glucosamine had been lost. This parallels the formation of pyrrole-2-carboxylic acid from sialic acids in similar conditions: only two carbons of the parent mannosamine remained in the product. As we shall see, these fissions can be formulated essentially as retroaldol cleavages. The loss of an acetyl group from acetylacetone in the process leading to 2-methylpyrrole is less obvious. Although an acetyl group can be cleaved by strong alkali from acetylpyrroles and even from acetylacetone, this does not happen fast enough at the pH used. Hence the acetyl group must be lost at an intermediate stage. The most rapid reaction between glucosamine and acetylacetone is likely to be the formation of the enamine, which then undergoes internal aldol condensation to a hydroxypyrroline. This can already give rise to an acetylpyrrole by the elimination shown, but the preferred mode, by about 5

to 1, seems to be the addition of hydroxyl anion to the acetyl carbonyl, leading to loss of acetic acid. This gives 5H-pyrrole which can generate the normal 1H-pyrrole not by prototropy but by a cleavage of the retroaldol type leading to loss of erythrose. There are minor ether-soluble chromogens in the concentrated reaction mixture and it is possible that some of them are 1H-pyrroles formed by 5H-prototropy and retaining the tetrahydroxybutyl group of the hexosamine. My analysis suggests that the favoured species should be 2-methyl-5-tetrahydroxybutylpyrrole, but nobody has purified the minor chromogens. There is always some unfinished business.

Around that time I became interested in the parallel Morgan-Elson assay, in which *N*-acetylhexosamines on heating in mild alkali solutions generating a purple colour with the Ehrlich reagent. Walter Morgan (1900–2003),[20] then working at the Lister Institute, had found that acetamidoacetaldehyde, which can be regarded as the parent *N*-acetylamino sugar, gave a similar colour after similar treatment; and he encouraged me to explore the chemistry. It turned out that the chromogen is formed in two steps. When acetamidoacetaldehyde is heated in concentrated aqueous sodium acetate and the solution is periodically extracted with ethyl acetate, colourless air-stable Ehrlich-negative crystals are obtained. If the heating is prolonged and extraction is carried out only at the end, a different crystalline compound is obtained, highly air-sensitive and Ehrlich-positive. The sodium acetate not only acts as a mild base but assists the extraction by a salting-out effect. The two compounds, both new, were identified as 1-acetyl-3-acetamidopyrrole and 3-acetamidopyrrole respectively; the latter was also obtained by Curtius degradation of the known pyrrole-3-carboxylic acid. 3-aminopyrroles are rare and this is still the simplest one ever made. It was also the first example of a Knorr synthesis succeeding with an acylated aminocarbonyl compound. I have a specimen of my 3-acetamidopyrrole in a sealed evacuated tube. It is still white after 42 years. But as an indication of Morgan-Elson chemistry it was another purple herring. Richard Kuhn and his collaborators did a fine bit of work on this, and they showed that the chromogens are not pyrroles but furans. They actually isolated and synthesized one of the chromogens.

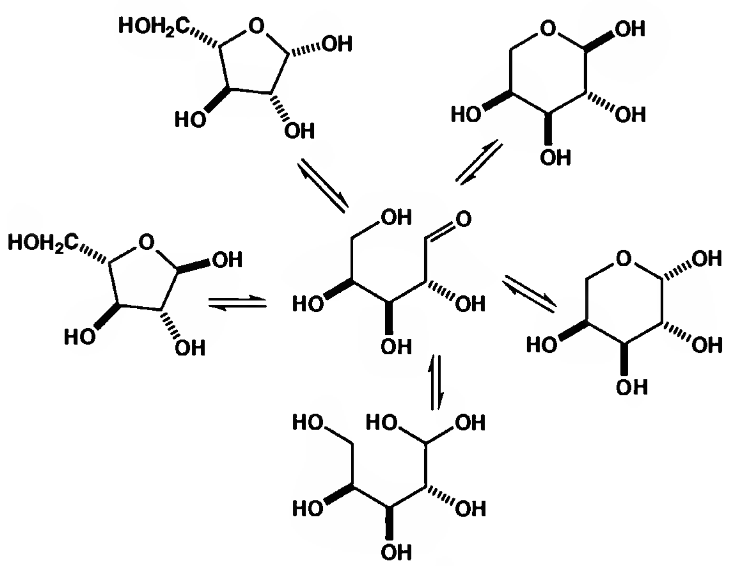
I thought that with this work I had finished with sugars. But during the last two years they have come back into my life. In the course of a survey of decarboxylation reactions I found two processes that I could not understand; one chemical, one enzymic. The chemical one is the catalysed decarboxylation of hexuronic acids. At about the same time as the work I have described, Zweifel and Deuel at the ETH in Zürich showed that galacturonic acid could be decarboxylated in astonishingly mild conditions: most remarkably, by heating with nickelous acetate in pyridine or with lead acetate in water (Figure 11).[21]



[20] https://en.wikipedia.org/wiki/Walter_Thomas_James_Morgan

[21] Zweifel and Deuel (1956).

Both reactions had been shown by Zweifel and Deuel to yield arabinose, which is nominally the product of simple removal of carbon dioxide from galacturonic acid. I have been investigating these catalyses. The nickel acetate reaction is showing every sign of being a monstrous red herring so far as the decarboxylation is concerned. The lead acetate reaction remains mysterious. But while I was preparing this lecture I re-read Kuhn's work[22] on the degradation of *N*-acetylneuraminic acid to acetyl-glucosamine. This was done with nickel acetate in pyridine and Kuhn actually mentions Zweifel and Deuel's work! Formally, the other product of this cleavage is pyruvic acid. But the only other product isolated was carbon dioxide and in a separate experiment Kuhn showed that pyruvic acid with nickel acetate and pyridine gave carbon dioxide but not acetaldehyde, the formal decarboxylation product. I had completely forgotten this experiment and I shall now repeat it and find out what is happening. But the work has also presented me with the problem of separating and identifying complex mixtures of sugars at the pentose level and this has led me to realize the deficiency of sugar analysis in its present state.



The Six Faces of L-Arabinose

Figure 12 is of the commonest aldopentose, L-arabinose, in aqueous solution. As you can see, the problem of converting quantitatively a hydrophilic, transparent, non-fluorescent, non-volatile, dynamic mixture of six components into a derivative that behaves as an individual and is measurable by UV absorption or fluorescence is not trivial, especially when one has up to six of these dynamic mixtures in the same solution. And if I succeed in my present attempt to do something about this, I shall at last have done something to please sugar chemists as well as to satisfy a continuing and probably lifelong curiosity about decarboxylations.

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Barron Field and the Myth of *Terra Nullius*

Justin Clemens

Associate Professor, School of Culture & Communication, The University of Melbourne

E-mail: jclemens@unimelb.edu.au

Abstract

While Judge of the Supreme Court of New South Wales, Barron Field published the first book of poetry in Australia in 1819. Field was also one of the founders of the Philosophical Society of Australasia in 1821.

The year 2019 will see another Australian bicentenary, the 200th anniversary of the first book of poetry published in this country. You could be forgiven for knowing neither the book nor its author; you could probably also be forgiven for not finding the event all that worthy of memorialisation, let alone celebration. Aside from a few specialists in colonial literature and a handful of historically inclined local poets, what possible interest could the (exceedingly) minor poetaster Barron Field — yes, his real name — and his *First Fruits of Australian Poetry* hold for contemporary Australians? Would they be of more interest if they were crucial evidence in the establishment of *terra nullius* in this country?

Field's own contemporaries tended to irritation and disinterest, when they weren't downright contemptuous. The wits had, so to speak, a field day with his poems — the man's name, unsurprisingly, providing rich soil for the punsters. As one anonymous squib from the 1820s, now preserved in the Mitchell Library, declares:

"Thy poems, *Barron Field*, I've read
And thus adjudge their meed —
So poor a crop proclaims thy head
A *barren field* indeed!"

Yet it wasn't only his enemies who mocked. Many of Field's friends weren't so keen on his verses, either. In his 1847 obituary for Field in the *New Monthly Magazine*, Horace Smith remarked of Field's poetry that "as truth is my friend, even more than Plato, I must confess my regret that he did not suppress them, for the gods had not made him poetical, his ear appearing to have been absolutely insensible to the requisite rhythm of verse". Such judgements have recurrently been made of Field's poetry over the subsequent centuries: even when his poems are (irregularly) anthologised, it seems to be more for their quaintness and historical import than for their inherent power or interest. No wonder Field has never really proven to be a key reference for Australian poets or their critics.

Then again, poetry may well have been Field's passion but it wasn't his day job. He was a lawyer, and, more to the point in this context, at the time of producing the book in question he was Judge of the Supreme Court of New South Wales. Appointed in May 1816, Field arrived in Sydney with his wife, Jane, in February 1817, and was immediately catapulted into the highest echelons of colonial society. His salary, supplemented by court fees, was very substantial; he also received a large grant of land in Cabramatta.

According to C. H. Currey's entry in the *Australian Dictionary of Biography*, Field dealt with 165 actions at law and 13 equity suits between April 1817 and January 1821; he also presided at the first sitting of a Supreme Court in Van Diemen's Land in 1819. In other words, Field was an exceedingly important personage at a moment in the fledgling colony of New South Wales when the vagaries of individuals could prove to have disproportionate consequences.

That said, Field doesn't seem to have been a terribly successful law man either. As John Byrnes rather understatedly remarks, "Field was not personally popular." It would probably be more accurate to state that he was outright loathed both by many of his colonial brethren and by the imperial governmental types with whom he had professional dealings. He fought bitterly with Governor Lachlan Macquarie, as well as with John Macarthur, whose appointment to the magistracy Field had attempted to scotch. Macarthur's enmity was such that he sent a threatening letter to Field before the latter's departure to England in 1824. "You will therefore, Sir," Macarthur writes, "be pleased to understand that I accuse you of having knowingly and deliberately committed an act which the manners of a gentleman forbid me to name even under the sanction of your example." This is, as Byrnes notes, an invitation to a duel. Given Macarthur's notorious irritability, it was clearly lucky for Field that he was on his way out at the time.

Yet it wasn't just Macquarie and Macarthur who were hostile to Field. Towering figures in the imperial administration at home were already entertaining doubts about his reliability. John Thomas Bigge, who had been dispatched to New South Wales by Earl Bathurst to investigate the functioning of the penal colony, and who ended up tabling three critical reports in the House of Commons, wrote of Field:

> "The convict part of the population of New South Wales view Mr Justice Field's administration of the law with sentiments of dissatisfaction. The free classes of the population … equally apprehend the effects of his violent and unforgiving temper, as well as of his personal prejudices, upon his future decisions … In my opinion, Mr Justice Field does not possess that degree of temper and deliberation necessary to conduct the judicial business of such a Colony."

The negative reviews don't stop there.

Perhaps the most eminent hater was none other than Benjamin Disraeli, who in 1830 pronounced Field to be "a bore and vulgar … a noisy, obtrusive, jargonic judge … ever illustrating the obvious, explaining the evident, and expatiating on the commonplace". A rebarbative and inveterate mansplainer, then, *avant la lettre*. It was no doubt in part due to his less-than-winning personality that Field spent the remainder of his career as an ineffectual judge in Gibraltar, retiring to England only a few years before his death. In a summation of Field's contributions in *Dewigged, Bothered, & Bewildered: British Colonial Judges on Trial, 1800–1900*, John McLaren concludes that

> "Field's record as a judge could best be described as mercurial, a reflection of his conservative belief system, a commitment to the culture of English law, and an opportunistic streak in his character … Field's counsel was not invariably sound or in keeping with the Colonial Office's understanding of the legal proprieties."

And yet, and yet … despite such continued bad press, Field was clearly not without certain impressive endowments. He was a direct descendant of Oliver Cromwell, a fact of which he was exceedingly proud. He had published the self-confessed first analysis of Blackstone's *Commentaries*, aimed at law students, and it went into many editions through the 19th century. He was theatre critic for *The Times*. He was friends with the great English Romantic critics Charles Lamb and Leigh Hunt, as well as an aficionado of the Romantic poets, especially William Wordsworth, of whom Field attempted a biography. The publication of the latter was, sadly for Field, vetoed by Wordsworth himself in 1840. Despite this setback, Field, who was a lifelong enthusiast for Elizabethan and Jacobean literature, prepared editions of Thomas Heywood and Thomas Legge for the Shakespeare Society. Lamb in particular seems to have had a real admiration for and friendship with Field. He reviewed *First Fruits* for the radical intellectual journal *The Examiner* in 1820, before dedicating one of his most celebrated essays, "Distant Correspondents", to the lawyer-poet in the same journal in 1822. He also informed Field of the esteem that Wordsworth and Coleridge had apparently shown for Field's poem "The Kangaroo".

Field clearly felt his verses were worthy of attention. The first edition of *First Fruits* in 1819, printed by George Howe in Sydney, and with the legend "Printed for Private Distribution" on the title page, contained two poems, "Botany Bay Flowers" and "The Kangaroo". In 1823, Field had a second, revised and expanded edition, which added further epigraphs and apparatus, as well as four further poems. Finally, in 1825, Field reprinted the poems as an "Appendix" to *Geographical Memoirs on New South Wales; By Various Hands*, a collection he edited, where they sit rather oddly with the journal entries and meteorological charts, the botanical descriptions and the imperialist opinionating.

It has to be admitted that the poems are pretty weird. Wreathed about with an ever-accreting and often-mystifying apparatus of epigraphs and erudition, their subjects are strange, their rhythms erratic, and much of their matter is flagrantly plagiarised. "Botany Bay Flowers", for example, is an extraordinary pastiche of Shakespeare, Milton and Wordsworth (and many more), where Field tells a tale of antipodean botanical adultery, in which he marries one flower, only to be seduced by another. As for "The Kangaroo", let me quote the first stanza:

"Kangaroo! Kangaroo!
Thou spirit of Australia,
That redeems from utter failure,
From perfect desolation,
And warrants the creation
Of this fifth part of the earth,
Which should seem an after-birth,
Not conceiv'd in the beginning
(For God bless'd his work at first,
And saw that it was good),
But emerg'd at the first sinning,
When the ground was therefore curst: —
And hence this barren wood!"

Whatever your feelings about their value, it's difficult to miss the strong satirical streak of these jaunty lines, along with Field's evident preparedness to pun on his own name and person. But it's also significant that Field is using words that, however straightforward they seem today, weren't so at the time. There was as yet — perhaps most notably — no country called "Australia". There were the colonies of New South Wales and Van Diemen's Land, yes, and the name of

Terra Australis was an ancient one in Europe. Governor Macquarie had certainly started to moot the idea, following the publication of Matthew Flinders' journals a few years previously, but most people were still referring to the landmass as "New Holland". As David Brooks remarks, "Field's use of the term 'Australia' … is arguably the first in poetry anywhere." In fact, Field uses the name throughout *First Fruits*, strewing his text with nominal and adjectival variations, even preposterously rhyming it with "failure" and "regalia".

With all this plagiary and playfulness about, coupled with the explicit colonial attitudinising, it's no wonder that the last few decades have seen a strong revivification of interest in Field's work, particularly among postmodern and postcolonial critics. Many eminent Australian writers and academics — A. N. Cousins, Michael Farrell and David Higgins, among others — have all written important pieces on Field. As Jaya Savige notes in his introduction to a special Australian edition of the prestigious journal *Poetry*, "To twenty-first-century eyes, Field's 'thefts' betray a poetics of appropriation and citation that wouldn't look entirely out of place in a Kenneth Goldsmith class." Yet none of the critics so far has asked the question: why *is First Fruits* called *First Fruits*? This might seem so obvious it's not worth asking: Field *knew* that he was publishing the *first* book of poetry on *Australian* soil, and was pompously belabouring this fact in the title, a fact supported by a number of features in the book.

Furthermore, the invocation of fruits is alerting us to the satirical nature of the poems: one of the (disputed) etymologies for the word "satire" is linked precisely to fruit. As the *Oxford English Dictionary* informs us: "According to the [Latin] grammarians *satura* is short for *lanx satura* … which is alleged to have been used for a dish containing various kinds of fruit." Moreover, if "first fruits" is an idiomatic expression denominating the earliest returns on labour, it is also, more pointedly, a technical term from ecclesiastical and feudal law. "First fruits", as Justice Field knew very well from his professional role, is a form of *income tax* to the *governor* of a territory.

As for his brief Australian sojourn, residues of Field subsist all over the place. Mount Field in Tasmania is named after him, while Cairncross Island in the Great Barrier Reef takes the maiden name of Field's wife. Soon after arriving in New South Wales, Field had edited *Memoirs of James Hardy Vaux*, famous, among other things, for its influential dictionary of thieves' cant: "A Vocabulary of the Flash Language". As a keen amateur scientist, Field observed, described and collected a wide range of important scientific and exploratory materials, much of which was published by John Murray in 1825 as *Geographical Memoirs on New South Wales; By Various Hands*. Due to such labours, Field has, according to Helen Hewson in the scientific plant journal *Telopea*, "two genera and one species … named in his honour" (*Fieldia australis*, *Fieldia lissochiloides*, and *Cassia barronfieldii*), as she offers the new combination of *Senna barronfieldii*. As if that wasn't enough, Field was critical to the establishment of the colony's first bank, when, according to C. H. Currey, he mistakenly advised Governor Macquarie that "the governor had power, under his commission, to grant a charter to the Bank of New South Wales". Established in 1817, this bank is still with us: it was renamed Westpac in 1982. At least one other further

legal judgement Field proffered in the course of his antipodean duties would have quite extraordinary effects upon the subsequent history of Australia.

In an important recent comparative study of the relation between colonialism and law, Stuart Banner has demonstrated that Field was decisive in the development of the specifically Australian application of *terra nullius*. One of the abiding puzzles regarding the centrality of this extreme doctrine in Australia is how it came to be established at all. After all, there was no question that the land was inhabited by the Australian Indigenous peoples, a fact acknowledged by all Europeans. If the doctrine had indeed been previously applied in certain colonial circumstances, by the 18th century the general policy was acquisition by forms of treaty and contract. Even if the latter were evidently iniquitous, they did not extinguish the facts of inhabitation. For Banner, then, at least four factors contributed to the doctrine in Australia: the land was sparsely inhabited, of another order than other places; the British saw no evidence of Indigenous cultivation of land; the Indigenous peoples were not a military risk of the same order as American or New Zealand peoples; and the Indigenous peoples showed no interest in European goods or trade. (Needless to say, each statement now appears highly contentious.) Hence, although Captain James Cook had expressly been ordered *not* to seize land from any inhabitants, by the 1780s Arthur Phillip was. In such fashion, *terra nullius* was *de facto* already enacted before it was formally declared as doctrine.

How then did *terra nullius* ever come to be *declared* as doctrine at all? "The first such statement," Banner writes, "appears to have been made in 1819, when a dispute arose between Lachlan Macquarie, the governor of New South Wales, and Barron Field, judge of the New South Wales Supreme Court, over whether the Crown, acting through Macquarie, had the power to impose taxes on the residents of New South Wales, or whether that power was reserved to Parliament, as was the case with taxes imposed on residents of Britain." Field, in a self-conscious replay of Sir Edward Coke's objections to the use of the Kingly Prerogative by James I — that is, to the very disputes that ultimately led to Cromwell's victory in the English Revolution — came down on the side of Parliament. If Australia had indeed been invaded, then Macquarie, as the representative of the sovereign, would have had that power, but, for Field, Australia was *freely settled*, and this was therefore a parliamentary matter. Earl Bathurst, secretary to the colonies, referred the matter to Samuel Shepherd and Robert Gifford, respectively the attorney and solicitor generals of Great Britain, who accorded with Field.

So as far as we know the first formal statement of *terra nullius* in this country derives from a tax dispute between the colonial governor of the penal colony and the bumptious supreme justice of that colony.

In common European colonial thinking, it was agriculture that established "a more permanent property in the soil" (to quote Blackstone). Field in *Geographical Memoirs* notes the ongoing displacement of Australian flora and fauna through the extension of European-style agriculture in the colony, not to mention the distress of the original inhabitants. Yet it was the agricultural civilisation and its fruits — taxes — that had primacy in European law, and Field could hardly have been more attentive to this fact. Banner's own comparative studies have induced him

to suggest that "where indigenous people lacked agriculture before European contact… the colonial acknowledgement of indigenous property rights was weaker or nonexistent". Let us add, following the work of Bill Gammage and Bruce Pascoe, among others: agriculture recognisable to Europeans, that is.

At precisely the same moment that Field is struggling over the legitimate grounds of taxation with Governor Macquarie, he produces these poems and this book. They barely resemble the poetry that he had previously published in the early 1810s, for example in *The Examiner*, nor his only other collection, *Spanish Sketches*, published in 1841. I propose that they can be understood in the context of this dispute, as if Field wrote them to say, "F*ck you, Macquarie, this satire is all the taxes you're getting from me." After all, the name "*Australia*" in Field's poetry doesn't designate a stable financial or tax entity, but functions as an expressly fantastic name drawn from an old European tradition of satirical takes on the Great Unknown Southern Land. Yet, in order to do so, Field had to offer what has proven to be a most iniquitous legal fiction.

So why return to Field today? In an epoch of decolonisation struggles globally, where memorials to such figures as Cecil Rhodes, American Civil War soldiers and, in Australia, Captain Cook have quite rightly become the objects of strenuous contestation, Field is at best a highly ambivalent figure. However, the politics of memory and memorialisation are paramount even in the most recondite academic researches. Every memorialisation is also invariably a form of motivated forgetting; every memorialisation reopens the question of whether there can be some restitution or reparation without the repetition of misdeed.

Field himself took the question of memorials very seriously. In the second edition of *First Fruits*, the newly added poems directly pick up a European history of memorialisation back to the ancient Greeks, in order to project a potential future of glory for the colony. As a member of the Philosophical Society of Australasia, Field was instrumental in sponsoring the first memorial erected in Botany Bay to Cook and Joseph Banks. Yet at least one of his sonnets, as Chris Healy points out in *From the Ruins of Colonialism*, "is rare in making explicit the violence of the initial British encounters with Aboriginal people and in remembering that the most material European remnant of the *Endeavour*'s brief stay in Botany Bay was a grave". What the Europeans brought to a locale that they had named precisely for its wild profusion of flora was the mark of death.

In his *Defence of Poetry*, the great Romantic poet Percy Bysshe Shelley resoundingly declared that "poets are the unacknowledged legislators of the world". This proposition has perhaps never been so directly true as in Australia. "Barren field" is a possible, if lateral, translation of one sense of *terra nullius*. It is as such that — unacknowledged yet omnipresent — the lawyer-poet Barron Field literally imposed his name on this land.

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Approaches to estimating pathway erosion on the Coast Walk, Royal National Park, New South Wales

Deirdre Dragovich* and Sunil Bajpai

School of Geosciences, University of Sydney, Sydney 2006, Australia

*Corresponding author.
E-mail: deirdre.dragovich@sydney.edu.au

Abstract

Natural area conservation has become more challenging with the worldwide growth in tourism. Increasing numbers of visitors lead to trampling of vegetation and soils with accompanying erosion and adverse ecological impacts. Although visitors accelerate erosion rates and can cause ecological damage, visitor amenity is in turn negatively affected by eroded pathways. Effective and timely management intervention requires information on which to act but conducting detailed track deterioration assessments places heavy demands on data sources and/or skills of personnel. A simple indicator of track erosion may provide a workable interim measure. In a preliminary investigation along the Coast Walk in Royal National Park, twenty sites were selected and assessed within a three-category erosion severity classification which included visitor-generated deterioration. Erosion losses were estimated using measured cross-sectional area and a soil erosion model. We found reasonable convergence between the erosion severity classification and results from the cross-sectional area and erosion model, although both needed interpretation of outlying data. A measure of maximum erosion depth emerged as the simplest general indicator of erosion loss. This indicator also places the least demand on personnel and data resources, an important consideration for budget-challenged park managers tasked with simultaneously providing environmental protection and visitor amenity.

Introduction

The worldwide growth in natural area tourism has led to increasing concern about potentially adverse and unintended environmental impacts, especially in protected areas. These impacts include pathway (track) erosion. Managing natural resources to meet the dual objectives of conservation and recreation provision is complex and dynamic (Bushell, 2003), and involves responding to environmental, policy, attitudinal, social and economic changes. Use of protected areas like Royal National Park for leisure activities has been encouraged by the State government (State of NSW and Dept of Environment and Climate Change, 2008) and similar approaches have been adopted internationally (e.g. Ministry of Tourism and Creative Economy of the Republic of Indonesia, 2012; Kingdom of Morocco, 2017). Policies are frequently framed within the paradigm of "sustainable tourism" or "ecotourism," generally with the stated intention of ensuring visitor satisfaction, minimal environmental impact, and economic and social benefits for local communities (e.g. Robinson and Picard, 2006; Director of National Parks Australia, 2011; UNESCO, 2016). Achieving such a combination of positive outcomes is challenging.

Pathway erosion results from visitor use of informal (Barros and Pickering, 2017) and formal access routes. Trampling of vegetation in the absence of raised or constructed walkways/surfaces is inevitable and impacts tend to increase with higher visitor (Marion and Leung, 2001; Nepal, 2003) and/or associated livestock numbers (Nepal, 2003; Ostoja et al., 2014). Well-known detrimental environmental consequences include accelerated rates of erosion and the degradation of soils, vegetation and water quality (e.g. Bayfield, 1973; Cole, 1995; Cole and Landres, 1996; Bhuju and Ohsawa, 1998; Arocena et al., 2006; Kissling et al., 2009). These impacts represent indicators which contribute adversely to ecological functioning, and also relate to erosional outcomes which disturb visitors' experience (Pietilä and Fagerholm, 2016). Consequently, erosion generated by vegetation loss and path deterioration becomes a concern for both ecosystem health and visitor experience (Lynn and Brown, 2003).

Management of pathways involves responding to existing deterioration (path condition), predicting future damage at these or other sites on existing or projected tracks, and monitoring visitor satisfaction (Table 1). Path condition can be assessed qualitatively in the form of a complete condition inventory, or by assessing deterioration at predetermined distance intervals, or by applying a combined approach of an initial qualitative categorisation followed by a quantitative investigation. Most quantitative methods adopt a sampling procedure within previously assessed condition categories. However, qualitative sampling points alone, or a combination of point-based qualitative assessment and measurement at within-category sampling sites, may both produce an under-estimation of path deterioration, as the extent of specific impacts between sampling locations is unknown (Marion and Leung, 2001). As would be anticipated, longer sampling intervals were found to reduce accuracy in estimating the extent of impacts (Leung and Marion, 1999).

The approximately 30-km long Coast Walk in Royal National Park has long been popular with visitors. It has a history of severe erosion, followed by pathway repair involving infilling of gullies, and subsequent re-erosion of the same sites. In 1981, for example, part of the Walk had been gullied to a depth exceeding 3 m (Young and Young, 2006); and in 1991 a gully more than 2 m deep — later infilled — had led to visitors creating an adjacent informal path (Figure 1). This continuing interaction of erosion processes with visitor impacts and management responses produces changing track conditions over time. Any pathway assessment thus represents conditions at the time of the study, and does not take into account the timeframe over which the observed erosional features developed.

Figure 1: A previously in-filled and subsequently deeply gullied section of the Coast Walk, 1991.

The Coast Walk had been deteriorating for some years before the government announced funding of $2 million over three years from 2014 to repair the most damaged sections (Galvin, 2014). As this activity commenced slowly, the current study was completed before these management interventions had altered the general condition of the Walk. By mid-2018, upgrading of a 9-km section was in the planning stage (Visentin, 2018). Conducting track deterioration assessments often involves heavy demands on data and/or skills and time of personnel, so a simple indicator of pathway erosion would benefit park management. The purpose of this investigation was to determine whether different approaches (one qualitative and two quantitative) to assessing pathway erosion produced similar and useful indicators of erosional loss. The approaches considered were qualitative categorical assessment, field estimates of erosion using measured cross-sectional area, and soil loss estimates using an erosion model.

Study area

Established in 1879, Royal National Park (RNP) is the world's second oldest national park and now covers an area of 15,080 ha on the southern fringe of metropolitan Sydney, Australia (latitude 34°04'16"S, longitude 151°03'21"E). Annual visitor numbers to RNP are estimated to be around 4 million, about 80,000 of whom walk all or part of the approximately 30-km-long pedestrian-only Coast Walk (Galvin, 2014). People are attracted to the natural scenic views, availability of family-oriented picnic and recreational facilities, and proximity to metropolitan areas with easy access by road and public transport. A high priority in the Park's most recently published Plan of Management (NPWS 2000) was to "restore the Coast Walk" and "review the system of walking tracks," some specific funding for which was finally provided in 2014–2017.

RNP experiences a warm temperate climate with moderate winter temperatures and warm summers. Over a 30-year period of records at Audley near the centre of the Park, most rainfall occurred during autumn (353 mm) and least during spring (215 mm). The annual average over the period was 1114 mm (Australia, Bureau of Meteorology, 2017). The Park has an internationally acclaimed flora collection with more than 1000 recorded plant species, of which 26 are classified as nationally rare or threatened (NPWS, 2000).

Geologically, the Park lies within the Sydney Basin, and lithology in this area is dominated by Hawkesbury sandstone, a fine-to-coarse-grained quartzose sandstone occasionally interbedded with shale. Underlying the sandstone is the Narrabeen shale series that emerge in the south. Both the sandstone and underlying shale are nearly horizontally bedded, so much of the Park forms a low plateau bounded by a cliffed shoreline with some rock platforms on headlands and occasional intervening beaches. With the exception of relic cliff-top sand dunes (Fairley, 2000) most soils have a loamy texture. Soils developed on shale have a heavier texture and range from loam to clay loams at the surface and medium to heavy clays in the subsoil. Where Hawkesbury sandstone forms the parent material, soils are mainly loams, ranging from coarse sandy loams to clay loams (Hazelton and Tille, 1990). Most soils in RNP have poor structure and are highly erodible.

Table 1: Approaches to managing paths in protected areas

Assessment (qualitative)	**Method / approach**	**Path characteristics**	**Author/s**
Qualitative assessment	4-category rating system	89 km	Nepal and Nepal (2004)
Qualitative inventory	Sample at each 100 m	4 km	Mende and Newsome (2006)
Qualitative broad assessment	Sample at each 20 m (recommended interval)	Can assess 5–7 km of track per day	Hawes et al. (2006)
Qualitative plus quantitative	5-category rating system; 5x6 sites measured	25 km	Gager and Conacher (2001)
Prediction (planning)	**Method / approach**	**Path characteristics**	**Author/s**
Predicting potential deterioration	Monitoring pre-classified path types (8 years)		Dixon et al. (2004)
Identifying relevant environmental variables	Analysis of field measurements	25 km; track type assessment for 1,700 km system	Gager and Conacher (2001); Hawes et al. (2013)
Using GIS to design 'optimum' path locations	5-category resilience classes (erosion susceptibility)	70 km^2 area	Tomczyk (2011); Tomczyk and Ewertowski (2013a)
Using GIS to plan visitor travel routes	Time and energy costs for visitors	22 km^2; 16 trails (182–8145 m long)	Chiou et al. (2010)
Monitoring — visitor behaviour and response	**Method / approach**	**Path characteristics**	**Author/s**
Noticeable erosion	On-site and web-based surveys	Eroded paths	Pietilä and Fagerholm (2016)
Erosion and diminished visitor satisfaction	Self-administered questionnaire	Impacted paths	Lynn and Brown (2003)
Erosion and visitor response	Questionnaire	Eroded paths, trampled vegetation	Dragovich and Bajpai (2012)

Methods and Materials

Erosion severity assessment

Physical measurement of the entire Coast Walk for a complete census of trail problems was not attempted. Rather, twenty sample sections (sites) were selected after reconnaissance which involved walking the length of the Coast Walk and ensuring that sample sites represented the range of erosion scenarios present. This directed-sampling approach was further constrained by sections where management had installed stairs or raised walkways, or concrete-hardened path surfaces; by rock outcrops; and by visitor use patterns, in that most walkers and erosion-affected surfaces are concentrated in two approximately 5-km sections at either end of the Walk where public access roads terminate. For practical purposes, it was assumed that recreational impact (trampling) was similar on both end sections of the Walk. A three-category erosion severity classification of high, moderate or low severity was devised for sections not modified by management. Based on field observation of erosion patterns, all sections categorised as moderately or highly eroded were measured. The remaining unmodified sections were classed as low severity and only some of these sections were measured.

Erosion severity assessment was based on the three key indicators, of channel (path) depth, surface roughness, and presence of stones, with higher values for measured or observed indicators signifying increased erosion severity. Measured channel depth was the primary indicator applying to all paths while uneven surfaces and stones were not always present. Channel depths were differentiated to broadly reflect the magnitude of water erosion in the form of sheet erosion, rill development and gullying (channels). A threshold channel depth of 30 cm was applied, as this represents the practical difference between rills and gullies (New South Wales Dept. of Primary Industries, 2015).

Rills or shallow channels of ≤30 cm in depth were arbitrarily sub-divided into those of <10 cm (low erosion severity) and of >10 cm (medium erosion severity), while gullied surfaces were those having channel depths >30 cm (high erosion severity) (Figure 2). Wind- and water-eroded sandy sections were characterised by pathway hollowing rather than channel development, but the same depth criteria for low, medium and high erosion severity were applied.

Surface roughness was categorised as even, uneven, or very uneven. Uneven walking surfaces are created by erosion, although shallow channels <10 cm deep allow path-

Figure 2: Examples of (A) high, (B) medium and (C) low erosion severity categories.

way surfaces to remain reasonably even, with sheet erosion being the dominant water erosion mechanism. As channels become deeper but occupy only part of the track, surface unevenness increases. Associated with this increased erosion is a higher probability of subsoils, bedrock or a lag of gravel or boulders being exposed, creating pathway surfaces that become increasingly uneven and potentially hazardous for walkers.

Evidence of uneven/unsafe walking surfaces, multiple tracks and near-path trampling of vegetation was described as user-generated degradation. As trampling may lead to soil compaction and increased runoff through reduced infiltration, a field penetrometer (Humboldt) was used to measure soil compaction within and adjacent to measured path sections.

Soil loss estimates

Cross sectional area (CSA) field measurements

Individual sites varied in size. Site length was defined as a stretch of the track where erosion severity was fairly uniform but became noticeably different from the adjacent upslope and downslope path sections. Site length ranged from 6.5 m to 30 m. Site width was defined as the distance between pathway edges or banks beyond which a marked disparity in erosion was evident, with site widths ranging from 0.92 m to 4.23 m.

Soil loss at each of the twenty sites was calculated using the Cross Section Area (CSA) method (Helgath, 1975; Gager and Conacher, 2001; Olive and Marion, 2009). A string was tied connecting four nails demarcating the site boundaries, with a further string inserted midway along the section where a soil sample was also collected. Depending on the depth of erosion, pathway samples might have represented the A or B horizon. Depth to the ground below each across-site string was recorded to the nearest 0.5 cm at 20 cm intervals. The area below each surface profile line was calculated by multiplying the total depths recorded by 20 and converting the result from cm^2 to m^2, and the total volume of soil loss was estimated by averaging the three values and multiplying by the site length. Soil loss per m^2 was estimated by dividing the calculated volume of soil loss by the surface area of the track surface (m^3/m^2).

Erosion modelling using SOILOSS

Estimated sheet and rill erosion rates were obtained by using SOILOSS (Rosewell, 1993), a program adapted from RUSLE for Australian conditions. The soil loss equation is:

$$A = R \times K \times L \times S \times P \times C \qquad (1)$$

where A is average annual soil loss in tonnes per hectare; R is rainfall erosivity; K is soil erodibility; L is length of slope; S is angle of slope; P is erosion control practices; and C is cover (vegetation) management.

Values for the individual parameters were determined by a combination of field recording, laboratory analysis and lookup tables. Rainfall erosivity (R) was based on the map accompanying the SOILOSS software (Rosewell, 1993). Slope length (L) and angles (S) were measured. For the soil erodibility (K) parameter, soil structure and permeability were assessed in the field, and laboratory results were used for texture and organic matter. Soil samples were analysed for texture using a Mastersizer 2000 and organic matter content was determined by loss on ignition (organic matter = loss on ignition × 0.7). Based on field observation, erosion control practices were assumed

to be absent (P =1 from lookup tables in Rosewell, 1993). Bare pathway surfaces were most common, but because incomplete grass cover or leaf litter was in some cases present on the less trampled edges of pathways, and rocks occurred on some paths, soil loss estimates were calculated using a value of *C*=0.45 (lookup tables in Rosewell, 1993). SOILOSS output was converted to kg/m^2 before comparing modelled SOILOSS estimates and CSA field measurements, with each retaining their respective measurement units.

Results and Discussion

Length, depth and width of measured sites

Individual sites varied within and between erosion categories in length, width and depth. On average, individual low erosion category sites extended over shorter distances, and were narrower and shallower than sites classified as having medium and high erosion severity (Table 2; Figures 2 and 3). Although sites in the medium erosion category were longer and wider on average than the highly eroded sites, the latter had a considerably greater average maximum depth (59.8 cm compared with 25.2 cm).

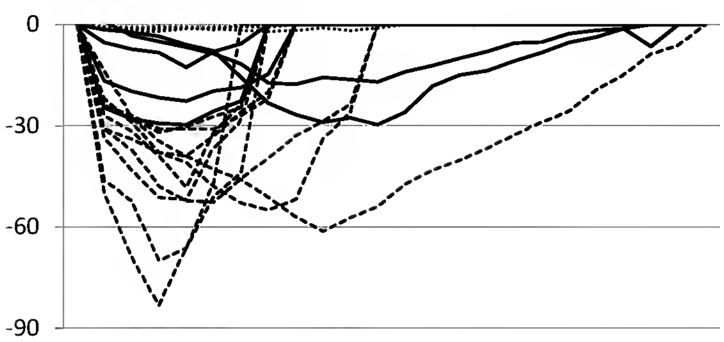

Figure 3: Path cross sections by erosion category (High = broken line; Medium=solid line; Low = dots, overlapping at this scale). Each path is represented by the average of three cross sections. Vertical scale in cm. Widest path = 4.6 m.

Soil compaction

The trampled pathway sites were often compacted and soils were not penetrable at nine of the 20 sites, with seven of these nine being high erosion category paths and one each of the medium and low categories. None of the 20 on-path sites was easily penetrable and in total these trampled sites recorded a mean value of 1.36 kg/cm^2 (Table 2). Untrampled areas were generally less compacted than paths and six untrampled areas had zero resistance recorded ('easily penetrable'). Two of the three trampled sites in the high erosion category were on sandy soils which would have contributed to pathway compaction being below that for adjacent untrampled areas (on sandy soils, footfalls churn the loose surface material rather than compacting it). This contrasts with loamy and clayey soils where trampling leads to compaction, a use-generated feature which compounds erosion susceptibility following initial vegetation loss.

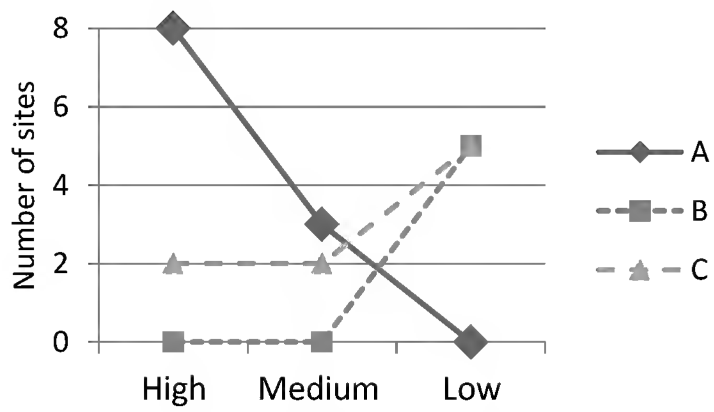

Figure 4: Use-generated degradation and erosion severity. A = multiple paths, vegetation trampled; B = minimal degradation; C = no extra tracks.

At each site vegetation in the central part of the path had been completely destroyed by trampling. With one exception, management interventions appeared on high and medium erosion severity sections (Table 3), suggesting that visual assessment was being

Table 2: Physical characteristics of sampled sites

Characteristic/ property	High severity	Medium severity	Low severity
Erosion severity criteria			
Channel depth (cm)	1+ XS value >30 cm	>10 cm and ≤30 cm	0 to <10 cm
Path surface	Very uneven	Uneven	Even
Stone size	5 – 10 cm	2 – 8 cm	<2 cm
Site dimensions (range)			
Average length (m) [(a)]	13.9 (7 – 30)	16.5 (6.5 – 20)	11.9 (6.5 – 17)
Average maximum depth (cm)	59.8 (34 – 85)	25.2 (15 – 30)	1.64 (1.2 – 2)
Average width (cm)	162 (92 – 423)	231 (110 – 395)	138 (101 – 205)
Average width:depth ratio	2.8 (1.2 – 5.9)	9.0 (4.3 – 15.2)	84.9 (67.3 – 116.7)
Sample sites — path slope and soils			
Mean path slope (range) (degrees)	7.2 (3 to 15)	6.6 (3 to 11)	1.4 (1 to 2)
Surface soil texture (no. of sites)	Sand (3); Loamy sand (5); Silty loam (3)	Loamy sand (5)	Loamy sand (4); Silty loam (1)
Mean gravel (range) (%)	13.9 (0.9 – 24.4)	15.2 (2.5 – 47.7)	11.5 (4.2 – 18.7)
Mean organic matter (range) (%)	2.2 (0.6 – 4.0)	2.5 (0.7 – 5.0)	3.0 (1.3 – 4.5)
Mean silt content (range) (%)	17.5 (3.7 to 36.9)	18.0 (13.6 to 22.7)	17.2 (9.7 to 31.8)
Mean clay content (range) (%)	2.9 (0.4 – 8.7)	2.5 (1.8 – 4.1)	2.2 (1.7 – 2.9)
Soil compaction (no. of sites)			
Trampled (not penetrable) (no.)	n=7	n=1	n=1
Trampled (no.) (mean kg/cm^2)	n=3 (1.3 kg/cm^2)	n=4 (1.5 kg/cm^2)	n=4 (1.25 kg/cm^2)
Untrampled (easily penetrable) (no.)	n=3	n=0	n=3
Untrampled (no.) (mean kg/cm^2)	n=7 (1.75 kg/cm^2)	n=5 (0.90 kg/cm^2)	n=2 (0.75 kg/cm^2)

(a) The length of path upslope of each site was not recorded. All other factors being equal, longer slopes have more erosion than shorter slopes

used by managers to identify potentially uncomfortable or hazardous pathway conditions for walkers. Only the low erosion sites recorded an absence of multiple tracks, uneven surfaces and walkers encroaching onto near-path vegetated areas (Figure 4). Natural waterlogging occurred at three sites and this can lead to uncomfortable walking conditions which may also encourage visitors to develop alternative routes.

Erosion estimates: SOILOSS

A summary of path slope angles and soil analyses for texture, gravel, organic matter and clay contents is provided in Table 2. Most soils were loamy sands, with three being classified as sands and the remainder as loamy sands or silty loams. Clay content of surface soils was generally low, between 0.4 and 4%, with only two samples recording amounts of more than 5%. Organic matter was also generally low, averaging between 2.2% and 3% for the three erosion categories. The gravel component contributed more than 10% at thirteen sites, with three of these recording amounts exceeding 20%.

Soil loss estimates for the high erosion severity group ranged from a minimum of 0.33 to a maximum of 7.20 kg/m^2/yr; for the moderate severity group, from 1.30 to 11.10 kg/m^2/yr; and for the low severity group from 0.22 to 0.78 kg/m^2/yr (Table 4).

Table 3: Use-generated degradation, path condition and management intervention

Use degradation and path condition	**No. of sites** [(a)]	**Erosion severity category** [(b)]
Track use pattern –		
Multiple tracks (5) plus initial phase (3)	8	5 H; 3 M
Walkers utilising near-path vegetated area	3	3 H
No additional tracks	9	2 H; 2 M; 5 L
Natural waterlogging	3	1 H; 2 M
Uneven/unsafe walking surface (on path, not adjacent)	11	10 H; 1 M
Minimal degradation	5	5 L
Management – degraded geoplastic and channel formation	2	2 M
Management – planks (now dislodged) along path banks	5	4 H; 1 M

(a) Sites may record >1 form of degradation
(b) Erosion severity category: H=high; M=medium; L=low

Erosion estimates: CSA measurements

CSA field measurements of estimated soil loss for each erosion category showed both within- and between-group differences (Table 4). Mean soil loss values increased 10-fold between the low to medium severity category, and another 2.8 times from the medium to high severity category. This regular increase in erosion estimates for low, medium and high severity sites contrasted with the pattern of SOILOSS estimates in which medium severity sites recorded the greatest amount of erosion. However, the patterns of between-method differences showed that for both CSA and SOILOSS, moderate and high erosion categories had substantially higher estimated soil losses than the low erosion category sites (Table 4). CSA registered the greatest soil losses for the high erosion category, contrasting with SOILOSS which recorded both the highest erosion loss and greatest between-site variability (95.6%) for the medium severity group.

CSA and SOILOSS estimates

Erosion estimates using the CSA and SOILOSS methods were compared by using erosion categories (Mann-Whitney U test, one-tailed) and linear regression of all-site data. Erosion losses between low and medium categories, and between low and high categories, had the same significant differences for both CSA (U=0, $p<0.004$ and U=0 p of 0.001 respectively) and SOILOSS. CSA recorded a significant difference between medium and high erosion categories (U=0 p of 0.001) but no significant difference occurred for SOILOSS (U=31, $p>0.05$).

Table 4: Mean and variability of erosion estimates using measured CSA (m^3/m^2) and estimated SOILOSS ($kg/m^2/yr$) grouped by erosion severity classes

	High severity	**Medium severity**	**Low severity**
Measured (CSA) soil loss in m^3/m^2 (sd)*	0.416 (0.143)	0.158 (0.071)	0.013 (0.005)
CV (%)**	34.5	44.9	38.5
Estimated SOILOSS in $g/m^2/yr$ (sd)	3.033 (2.321)	5.400 (5.162)	0.542 (0 276)
CV (%)	76.52	95.6	50 9

* Standard deviation in parentheses
** Coefficient of variation

When CSA and SOILOSS erosion results were compared by ignoring erosion severity categories, no significant correlations were noted between the two methods for all sites

(R^2 = 0.015, p >0.1) (Figure 5) or for the ten high severity sites (R^2 = 0.001, p>0.1). Two pairs of sites registering major anomalies were identified in Figure 5: sites 6,7 and 16,17. Excluding these outliers, the remaining 16 sites recorded a significant linear correlation (R^2 = 0.600, p<0.001). The outliers registered the highest values for SOILOSS (6,7) and CSA (16,17) (Table 5) and thus warranted further investigation as contributors to identifying potentially high-risk erosion sites. Two possible explanatory environmental variables were considered, namely slope and soil texture.

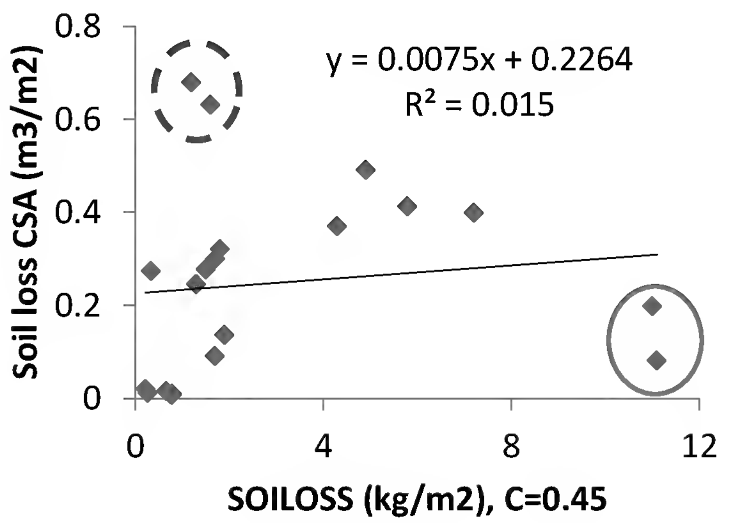

Figure 5: SOILOSS and CSA at all sites (n=20). Circled outliers 6,7 = solid line; outliers 16,17 = broken line.

(a) Slope

Average slope angles for sites categorised as high, moderate and low erosion severity were 7.2, 6.6 and 1.4 degrees, respectively (Table 2). In this study, slope angle explained more than half the variation in estimates of soil erosion using SOILOSS (R^2 = 0.61 p<0.001), partly because slope is a variable included in the equation; but slope was not a significant variable using the CSA method (R^2 = 0.13 p>0.1). Sites 6 and 7 were categorised as medium erosion severity (Table 5), even though they occurred on steeper slopes (11 and 10 degrees respectively) than high erosion severity sites 16 and 17 (both on slopes of 3 degrees). In accounting for the lower than expected measured soil loss (CSA) of outliers 6 and 7, two factors may have contributed: these sites had stones of varying sizes adjacent to and within the path; and management had installed a now-degraded geoplastic material over the trampled area. In estimates using SOILOSS, no adjustment was made for management/natural factors — thus SOILOSS estimates on these slopes were high (Figure 6a). The CSA method reflected the depth-limiting outcome of both stones and management intervention, producing erosion estimates that were more comparable to sites on slopes of less than 5 degrees (Figure 6b).

Table 5: Characteristics of 'outlier' sites

Site no.	Erosion severity category	Erosion estimate high for:	Average slope (^{0})	Mean max. depth (cm)	Stones present	Intervention (geoplastics)	Silt content (%)	Penetrometer (trampled) (kg/cm^2)
6	Medium	SOILOSS	11	25	Yes	Yes	<25	1.0
7	Medium	SOILOSS	10	15	Yes	Yes	<25	1.5
16	High	CSA	3	70	No	No	>30	Not penetrable
17	High	CSA	3	85	No	No	>30	Not penetrable

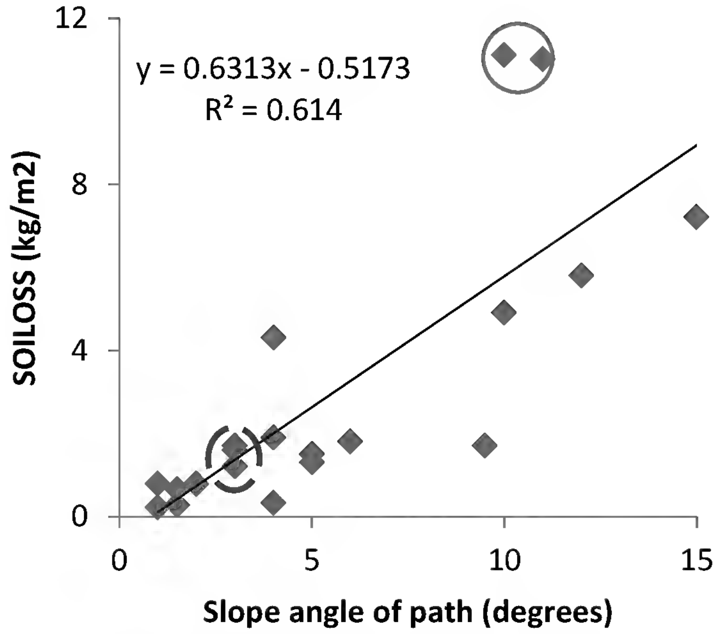

Figure 6a: SOILOSS and slope angle.

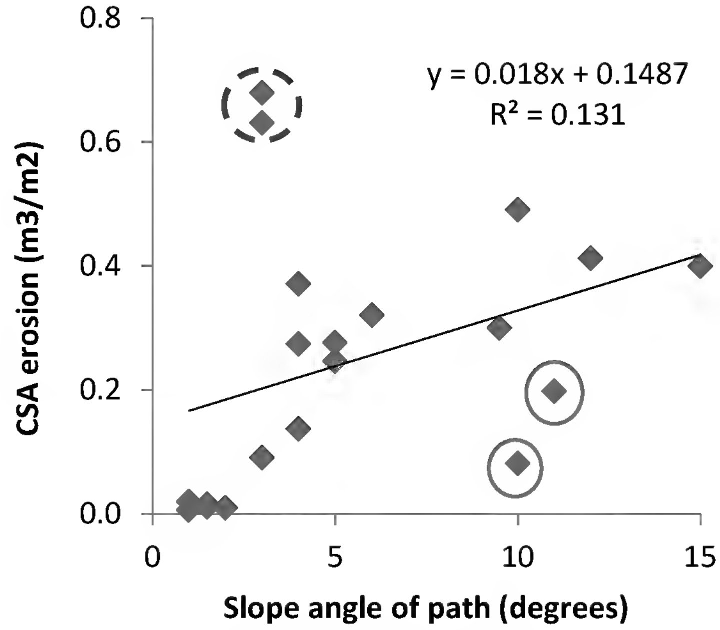

Figure 6b: CSA and slope angle.

Circled outliers 6,7=solid line (geoplastics); outliers 16,17=broken line (high silt content)

(b) Soil texture

Sites 16 and 17 were categorised as having high erosion severity (Table 5) and, although located on gentle slopes, they nevertheless registered high CSA estimates but did not appear as outliers for SOILOSS. For these sites the substantial silt content (>30%, the highest of all sites) is the most probable explanation for the high erosion estimates for CSA. Silty soils are highly erodible (USDA Natural Resources Conservation Service, no date) and silt can maintain steep cohesive path banks, which in sites 16 and 17 resulted in deep and narrow sections having the lowest width to depth ratio (1.2 and 1.3) of all sites. Trampling had compacted the pathway surface making it impenetrable with a hand-held penetrometer (Table 5) and had thereby increased overland flow by reducing infiltration. In SOILOSS estimates, the silt factor would have been offset by the gentle slope angle.

Another three sites recording loose sandy soils, which are readily detached and transported by water and wind, were categorised as having high severity erosion but did not contribute to outliers (sites 1, 4 and 11). Slope angles for these sites were 4, 12 and 10 degrees respectively, indicating that regardless of slope, sands are likely to be highly susceptible to pathway erosion. Trail width:depth ratios were 4.1, 3.3 and 1.8 respectively, compared with a mean of 2.8 (Table 2). At these three sites, depth of erosion (34, 60 and 57 cm respectively) was associated with reasonably wide trampled areas (mean of 147 cm).

CSA, SOILOSS and maximum path depth

Measured CSA represents erosion that has occurred. Modelling may describe existing erosion and predict future outcomes, as well as being applied as a comparative benchmark against which to assess the effectiveness of conservation measures. Both measured and modelled approaches have value within their respective contexts and yield useful information when combined. However, the relative influence of individual factors, and therefore their management value as erosional indicators, will vary between specific physical environments and visitor usage patterns. In relation to variables considered here, the erosional detail provided by CSA shows a

strong correlation with mean maximum path depth (R^2 = 0.869 $p<0.001$, Figure 7), a single measure which is simple and relatively quick to estimate and record. A much weaker but still significant correlation (R^2=0.487 $p<0.001$) was noted between CSA erosion and maximum depth:width ratios. Maximum path width was not significant ($p>0.5$) as a single variable for the CSA method (R^2=0.018), and none of the depth, width or depth:width ratios correlated with SOILOSS estimates (R^2=0.037, R^2=0.000 and R^2=0.164 respectively).

The advantages and disadvantages of the methods of assessing pathway erosion loss examined here are summarised in Table 6.

Table 6: Advantages and disadvantages of methods used for estimating pathway erosion

Estimation method	Advantages	Disadvantages	Examples
Use-related degradation	Trail modifications easy to observe and record (qualitative assessment).	Exact edge of trampled vegetation may be unclear in places; comparability between different observers' categorisations would need to be checked.	Explanatory variable often incorporated within qualitative and quantitative methods.
Qualitative erosion severity assessment (sampling frequency dependent on path length)	Simple classification criteria can be used; low cost to implement; observers readily trained; depth and other estimates easily made; only one person required to make assessment.	High input of labour time for lengthy tracks; boundaries between simple categories in erosion severity classification may be uncertain; extent of impacts between sampling points unknown.	Nepal and Nepal (2004); Mende and Newsome (2006); Hawes et al. (2006); Marion and Leung (2001)
Quantitative erosion severity assessment (cross sectional area measurements)	Simple to set up and record.	More time-consuming than qualitative assessments; precision of measurements may be affected by lateral slope	Jewell and Hammitt (2000); Olive and Marion (2009); Gager and Conacher (2001)
SOILOSS/RUSLE modelling	Effective erosion indicator at a broad scale; knowledge of the model allows for explanation of variations in local output values	Broad-scale modelling may not adequately represent track deterioration at a local scale; the model incorporates only water erosion and excludes wind erosion	Kuss and Morgan (1984); Vinson et al. (2017)

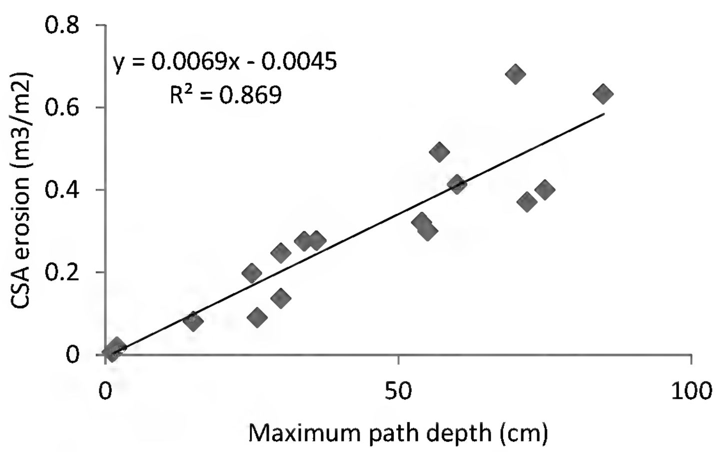

Figure 7: Mean maximum path depth and soil loss (CSA).

Caution needs to be exercised in relation to potential limitations of extrapolating our study to other environments. First, the number of sites we investigated is small statistically, and such a sample may produce anomalous outcomes which are not supported by larger data sets. However, we noted congruence between approaches that were expected to reflect that relationship and were able to point to factors likely to generate anomalies even in the small data set.

Second, variables other than those considered here may be critical to erosion in other environments, requiring adequate local knowledge to assess which physical variables are relevant and whether maximum track depth would be a suitable erosion indicator. Third, even though maximum track depth data correlated significantly with measured CSA erosion, the depth data were means of three CSA values recorded at each site and therefore underestimated individual maximum depths at specific points within each site. Finally, the composition of the visitor population may change over time, with accompanying differences in attitudes and behaviour, leading to altered patterns of use-generated erosion.

Conclusion

Soil erosion is a key variable in assessing track deterioration although it is not necessarily the sole useful indicator for pathway management in all environments. Irregular shallow bedrock, waterlogging or tree root exposure may also be responsible for walking discomfort — but not substantial depth increases — that prompt formation of multiple tracks and associated vegetation trampling. In this study, estimated quantity of soil loss was prioritised over walking comfort in the rare instances where either rock exposure or waterlogging was present.

The presence of pathways is an essential component of both proactive and reactive management of protected areas which contain valuable or rare ecosystems and are accessible to the general public for enjoyment of a variety of leisure activities. In the study area we applied several approaches to assessing pathway erosion and these converged to produce a reasonably consistent result. At the time of the study, sites classified qualitatively as having high erosion severity and without management intervention had the most visitor-generated pathway degradation and, on average, the greatest amount of measured soil loss (CSA) as well as high estimated (modelled) erosion rates. Both erosion indicators were more reliable when slope and soil texture factors were incorporated. We found that qualitative categorical assessment of pathway degradation can be combined successfully with use-generated deterioration and the erosion-estimating approaches of CSA or an erosion model, with field knowledge of physical track conditions being necessary for the interpretation of both qualitative and quantitative information. Maximum path depth provided a simple single indicator of relative erosion losses.

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Introducing Julian Tenison-Woods and the mines and minerals of the Malay Peninsula

Roderick O'Brien

School of Management, University of South Australia, Adelaide, Australia

E-mail: Roderick.O'Brien@unisa.edu.au

Abstract

In 1883 and 1884, the Australian pioneer scientist and priest, Julian Tenison-Woods, conducted geological, zoological, and geographical research in the area which is now Malaysia. During a visit to Hong Kong in 1885, Tenison-Woods lectured on the mines and minerals of the Malay Peninsula, which was reported at length in the local *China Mail* newspaper. The Governor, Sir George Bowen, presided at the lecture, and sent the newspaper text, along with his own comments, to the Colonial Office. The editor of the *Journal & Proceedings of the Royal Society of New South Wales* has included these materials in this issue, and this short text is an introduction.

The Source

Recently the *Journal & Proceedings of the Royal Society of New South Wales* published a government report by pioneer scientist Julian Tenison-Woods on the mines and mineralogy of Malacca (Tenison-Woods 2017). That report on Malacca was one of the fruits of the research conducted by Tenison-Woods in the Malay Peninsula in 1883 and 1884. After his work in that area, Tenison-Woods travelled in south-east and east Asia, including four visits to Hong Kong (O'Brien 1984). During one of these visits, in February 1885, Tenison-Woods delivered a public lecture on the mines and minerals of the Malay Peninsula. The lecture was reported in the local newspaper, the *China Mail.* The Governor, Sir George Bowen, presided at the lecture, and the next day he sent a copy of the newspaper report with a covering letter to the Colonial Office (Bowen 1885).

Tenison-Woods was keen that his researches should be published, and in a timely manner. Some of his publications on his work in the Malay Peninsula are readily available (O'Brien 2017), but others have remained hidden in colonial-era documents. Your editor has already generously published the report on Malacca (Tenison-Woods 2017), and now publishes the newspaper text and Governor Bowen's letter, taken from the files of the Colonial Office. A newspaper report on a public lecture has its own flavour, including notes of audience participation and the introductory and concluding remarks. The occasion also provides an insight into the financial links between Hong Kong and the mines of the Malay Peninsula. This lecture bears comparison with another lecture given in Perak in 1884, over two sessions (Tenison-Woods 1884). While the science is consistent, in Hong Kong there is a different audience, and Tenison-Woods expands his lecture to include economic issues of interest to the Hong Kong listeners.

Historian Christopher Munn of the University of Hong Kong has kindly and skilfully brought to contemporary attention the

Colonial Office files, and has helped with background information on the Governor, Sir George Bowen, whose participation is reported.

Julian Tenison-Woods: Religion and Science

Because an introduction to Fr. Julian Tenison-Woods is available with the report on Malacca (O'Brien 2017), he need not be introduced again.

However, the opening by Sir George Bowen specifically raises the question of science and religion, and was promptly reported in England, thus there is an opportunity to examine Tenison-Woods' own views. Both science and religion are interwoven in Tenison-Woods' remarkable life. In 1857, his first scientific publication on Australian geology was published in Victoria, dealing with metamorphic rocks in the Clare district north of Adelaide (Tenison-Woods 1857). The Clare district was the location of Sevenhill College, where Tenison-Woods undertook brief training with the Jesuits, and 1857 was also the year of his ordination as a Catholic priest, and his assignment to his first parish of Penola in the south-east of South Australia. He spent ten years as the parish priest of Penola, and by 1862, published his first book-length study of geology, principally on the region of his ministry (Tenison-Woods 1862). Tenison-Woods ended his first paper with a description of the sea "leaving rocks and stones to tell to man, the magnitude and power of the earth's Great Framer (Tenison-Woods 1857, 176). And in his first book, he wrote: "Let us congratulate ourselves that Geology displays as much the wonders of the Creator as its sister sciences, Chemistry, Mineralogy, or Botany as they bewilder us with visions of God's immensity..." (Tenison-Woods 1862, 350).

He remained a committed Catholic priest and a keen scientist for the rest of his life. Mary MacKillop (latterly Saint Mary of the Cross) wrote that his missionary work in Tasmania gave him an opportunity to restart his scientific work. "A geologist or botanist finds many things to interest him in Tasmania, and Father Tenison Woods, being both, was naturally much pleased. In his spare moments, as usual, he made notes and wrote scientific papers .... It was a long time since he had published any scientific writings; they were continued now as long as he lived, but they never interfered with his religious duties" (Mother Mary of the Cross MacKillop, 2010, 203).

Tenison-Woods did not respond to Governor Bowen's remarks on religion. After all, he was the guest of the Governor. Privately, he wrote a few days afterwards to his friend William Archer in Australia: "I had reason however to remember you without this reminder for I have been staying with Sir Geo. Bowen who more than once mentioned your name in connection with his reminiscences of Victoria. I have found him most kind and hospitable, but from the enclosed extract I send you it appears that he does not succeed in pleasing everyone. He took the chair at a lecture I gave when as you perceive his remarks were not in the best of taste — a weak point in his speechifying which was well-known to you" (Player, 1983).

Sir George Bowen, Governor

Sir George Bowen was Governor of Hong Kong from 1883 to 1887. An Oxford graduate in classics, he regularly displayed his classical scholarship. An anonymous note on the Colonial Office file reads, "I haven't come across copies of the Governor's writings without some Greek or Latin verse in it!" In the Ionian islands he began a career of over-

seas service which included office as the first Governor of Queensland (1859–1868), and Governor of Victoria (1873–1879). Bowen's hospitality and tactlessness were both well-known: opinions about his ministry as governor varied (McConnel 2013).

In his report to the Colonial Office, Bowen highlights two aspects of Tenison-Woods' lecture which he considered important. The first was political, as Bowen noted the disputed boundary between Siam (Thailand) and the Malay states. The second was economic, as Bowen noted Tenison-Woods' cautious endorsement of the methods of Chinese labour as more appropriate than the methods of European technology.

It is not clear when Bowen and Tenison-Woods first met. Bowen, in his introduction, says that Tenison-Woods reminded him "that he was my guest nearly a quarter of a century ago, when I was first Governor of the great colony of Queensland." But in 1860, Tenison-Woods had not long commenced his "ten years in the bush" at Penola, and his biographers do not mention travel to Queensland.

Bowen is glowing in his introduction of Tenison-Woods, complimenting him on his scientific work, and on his book on exploration (Tenison-Woods 1865). In response, Tenison-Woods said that "he had been a witness to the great efforts His Excellency had made to forward anything which favoured the advance of science, whether geological or geographical in [the Australian] colonies." One biographer records that Bowen encouraged the exploration of northern and inland Queensland, and that he accompanied an expedition which led to the formation of a coaling station and settlement at Cape York, in Queensland's far north (Carlyle 1901). He was the first president of the Philosophical Society of Queensland (later to become the Royal Society of Queensland) which had been founded shortly before his arrival. (The Society first met on 1 March 1859, just before the colony was proclaimed in London on 6 June 1859. Bowen did not arrive until December.) Bowen's lengthy collection of letters and papers, published towards the end of his career, makes no mention of the advancement of science in Hong Kong (Bowen and Lane-Poole, 1889). The establishment of the Hong Kong Observatory had been approved in 1882, and Bowen is remembered for implementing that decision in 1883 (Hong Kong Observatory, undated).

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The mines and minerals of the Malay Peninsula

By the Revd. J.E. Tenison-Woods, FGS, etc.

Abstract

At the invitation of Sir Frederick Weld, the Governor of the Straits Settlements from 1880 to 1887, and Sir Hugh Low, the British Resident of Perak from 1877 to 1889, Fr. Julian Edmund Tenison-Woods spent six or seven months in 1884 exploring through the state of Perak in the Malay Peninsula, then visiting Malacca and Selangor. While his exploring ranged across a number of sciences, he is remembered particularly for his geological work. During a visit to Hong Kong in 1885, he delivered an address on the "Mines and Minerals of the Malay Peninsula," which was recorded by the local *China Mail.* The journalist adds something of the flavour and humour of the afternoon, with local references to investors, and by providing the introductory remarks of the Governor of Hong Kong, Sir George Bowen. The text which follows is taken from that newspaper of 3rd February 1885.

An interesting lecture on the Mines and Minerals of the Malay Peninsula was delivered yesterday afternoon by the Rev. J. Tenison-Woods, in St. Andrews Hall. There was a large attendance: His Excellency the Governor presided, and among those present were His Excellency Vice-Admiral Dowell, K.C.B., Mr. Justice Russell, the Hon. W. Keswick, the Hon. A. Lister, and several ladies.

HIS EXCELLENCY, in opening the proceedings, said — Ladies and gentlemen, I have been requested to take the chair this afternoon on the very interesting occasion of the delivery of a lecture by the Rev. Julian Tenison-Woods on the "Mines and Minerals of the Malay Peninsula." As I am informed, this is a matter of practical and financial, as well as of scientific interest to some gentlemen in this colony — [laughter] — who have advanced funds for the exploration of the mineral resources of the Straits Settlements. If this be so, I earnestly trust that the patriotic efforts of those gentlemen to promote the cause of science will be rewarded by financial success; that they will be rewarded not only by advantage to their minds but also to their pockets. [Applause]. Of one thing I am quite sure, and that is, that they will get the most correct and useful advice from Mr. Tenison-Woods — [applause] — whom I have known for twenty-five years as an eminent geologist and mineralogist, and as the author of one of the best books on the exploration of Australia. My rev. friend has reminded me that he was my guest nearly a quarter of a century ago, when I was the first Governor of the great colony of Queensland, and I often heard of his reputation afterwards while I was Governor of New Zealand and Victoria — in fact during the whole twenty years that I passed in Australia as the Representative of the Queen successively in three of the greatest provinces of the Empire. [Applause]. I have therefore great pleasure in introducing Mr. Woods to this meeting. And here let me observe that the lecture of this day presents a very interesting subject of reflection. Here we have a practical proof that religion has no longer any fear of science. [Applause]. We see a Roman Catholic clergyman about to lecture on what was once considered the dangerous science of geology, and I am surprised that we have not the Bishop

ready to applaud him, but I am sure that it must be owing to some accident that my friend Bishop Raimondi is not here today. [Applause]. In the sixteenth century, as we all know, the great astronomer Galileo was persecuted because he contended that the earth goes round the sun, and until quite lately geology was considered a more irreligious science than astronomy. This feeling was not confined to the Church of Rome. At the end of the last century an eminent Bishop of the Church of England ridiculed the pretensions of geologists — and we know that ridicule is often a more dangerous weapon than hatred; as Horace says:

Ridiculum acri fortius ac melius magnas plerumque secat res.

— by saying that for a man crawling on the face of the earth to pretend that he knew what was going on in the interior of our planet was like a gnat on the shoulder of an elephant pretending that it knew what was going on in the bowels of the huge animal. [Laughter]. But behold what progress! Here we have Mr. Woods, at the end of the nineteenth century, about to tell us living in Hong Kong what is going on in the bowels of the Malay Peninsula, some three thousand miles away. [Applause]. Seriously, ladies and gentlemen, in the entire history of science there is nothing more remarkable than the progress of geology during the present century, or I will say during the last fifty years. So it is more or less with all the sciences, but I think the progress of geology is the most remarkable of all. But, like all other sciences, though it has achieved many victories, it has still many victories to achieve. That grand old philosopher, Sir Isaac Newton, on his deathbed, said that whatever might be thought by others of his great discoveries in natural science, he himself only felt that he had been like a child gathering shells on the shore of the eternal ocean of truth and knowledge. [Applause]. The field of science is like one of those vast Australian forests which Mr. Woods and I know and love so well, in which the more trees are felled the greater appears the expanse of wood around. Without further preface I now introduce to you the Rev. Mr. Tenison-Woods. [Applause].

The Rev. J. TENISON-WOODS said that it was due to his hearers, and in a certain sense due to himself, that he should explain as briefly as possible some of the qualifications he had for dealing with this subject, and why he had approached the subject at all in the lecture which he now proposed to give. He had lived for a long time, as His Excellency had informed them, in Australia, but His Excellency had not informed them, what he might now add, that during the time he had lived there he had been a witness to the great efforts His Excellency had made to forward anything which favoured the advance of science, whether geological or geographical, in those British colonies. In the course of his missionary duties he had explored a great deal of country which had never before been trodden by the foot of civilised man, and as he was always very much interested in geological subjects, and had visited most of the mineral-producing countries in Europe, he wrote down his impressions of what he then saw. He thought no more charming study could be imagined than a new country in which something is discovered which may largely influence the destiny of the colonies, and perhaps to some extent the history of the world.

And so, having seen a good deal that would interest the public, he published some of what he saw, at first on his own account and at his own cost. One or two of his first

works were geological, and these, in course of time, led him to be regarded as an authority on the subject, and the Government assisted him afterwards, and enabled him to publish accounts of many of his researches. In the course of time, he was invited to visit and report upon many mineral districts that had been discovered, so that the Government might have the opinion of an impartial and independent witness on the value of the deposits.

After many years had been spent in this way, during which he might say there were few mineral districts in the whole of Australia which he had not visited, he went on a visit to an old friend of his, Sir Frederick Weld, and while at Singapore he was invited by Sir Hugh Low to visit the mineral districts in the state of which he was Resident, and on the part of the Government His Excellency promised to see that he was taken about to every place he wished to see free of all expense, provided that he would give them a report. This he did. He spent six or seven months exploring through the native state of Perak, during which explorations he had the pleasure of becoming acquainted with some of the features of the country not previously made known. Subsequently he visited Malacca, and afterwards went through some of the mining districts of Selangor. Thus, he became intimately acquainted with all the mineral producing deposits of the Malay Peninsula. From there he went to Sumatra and Bintang, and the celebrated tin districts under the Dutch Government. Thus, he became not only acquainted with the tin deposits of Australia, which mineral ranked second as a production of that island only to gold, but he also became acquainted, and had means of comparing what he saw in Australia with what was to be seen in the Malay Peninsula. So much for his qualifications for dealing with the subject, and now as to his reasons for giving this lecture.

A great deal of misunderstanding existed with regard to the minerals of the Malay Peninsula. Very little was known about them, though he believed there were many in Hong Kong who thought they knew a great deal too much. [Hear, hear]. But for all that, he thought that if they had a little more explanation they would not take a despondent view they now seemed apt to take as to the future of the mining districts. What he proposed to do was to avoid as much as possible all technical matters, and give them as clear an idea as he could of the nature of the tin deposits of the Malay Peninsula. Though the object to which he should thus confine himself was a small one, he should have to choose very carefully among a multitude of subjects connected with it in order to fit it within the limits of a brief lecture.

From what he had seen and all he had read of the tin deposits of the Malay Peninsula, he concluded that they were, without exception, the richest in the world. He was aware that there was a prevailing impression here that this was not the case, but still it was perfectly true. There was no more widely distributed deposit of tin than in the Malay Peninsula, at least not of that particular kind of tin deposit which was termed "stream tin." They were very widely spread, and the deposits in themselves were practically inexhaustible. The lecturer proceeded to show by a map the peculiar formation of the peninsula remarking that its shape was singularly developed. There was an immense range of mountains, a few of which had been explored, and some of which he was the first to explore, while others, much higher, had been effectively explored by a French gentleman who had

since gone home. Some of the mountains reached a height of 10,000 feet, and there were, he believed, some even higher than that. These mountains, in about the centre of the peninsula, began to decline until they came to a part at which there was so little elevation that boats might be dragged from one river to another on the opposite side. It was there that the richest part of the tin deposit was to be found; there had been the greatest disturbance and there was the greatest mineral richness.

In a disputed country between the territory of Perak and Siam there were tin veins. He should speak briefly about those veins, and he might say that this was the only case he had met with where there were veins of tin in the peninsula; elsewhere, in the south, it was in alluvial deposits. This was a very important thing to bear in mind, because he had met in Hong Kong several persons who had asked him if there was not something peculiar about the tin deposits there. To these he would answer that there was nothing peculiar at all about them, they were exactly like stream deposits all over the world — no difference. He had been asked something about pockets; he knew pockets were a subject in connection with mines about which people were particularly tender — [laughter] — but there were no such things as pockets as they were generally understood which made them different from deposits in other parts of the world. The deposits of tin had been accumulating for ages from the wearing down of the granite rocks. He was also asked if there were no main sources from which the tin was derived, and he answered, "No, nothing of the kind was found in what was known as stream tin, as far as his experience went." It was most important to bear this in mind, that, wherever stream tin was discovered, veins were not found, and where veins were found, there was no stream tin. Had time allowed, he could have explained the reason of this, but he had now to confine his attention to what stream tin was.

Let them suppose that they had in the mountains of the Malay Peninsula the representation of those forces which had upheaved their rugged summits, which had in the course of time, through the slowly acting forces of the weather, worn into peaks and gullies as they were now seen. The rev. gentleman said his hearers must excuse him if he appeared to enter into explanations of things that seemed self-evident; as he proceeded the reason would be apparent. This decomposing and wearing down of the granite by the action of the weather and owing to the iron contained in the feldspar being easily rusted, and thus the granite was disintegrated till nothing was left to represent it but heaps of fine sand. With this sand, there was washed down by the force of water grains of tin that had been contained in the granite. Whatever was contained in the granite was washed down into the valleys, and a great deal of alluvial granite was carried out to sea in the form of a fine mud which discoloured the water, but was finally deposited on the coast, and was the cause of those immense mangrove flats and mud islands that were to be found about the Malay Peninsula on the west coast. The lighter portions were carried away the furthest and the heavier portions remained at the foot of the hills, and it was with the latter that the tin was found. It was found at the bottom of the deposit, and it might be said that if it was thus deposited it must have been said that only the surface of the granite which was rich in tin, and it was washed down first, and subsequently covered with alluvial sand. But it would not

be right to argue in this way. The granite and tin had been mingled together so generally that probably a very minute examination of the granite would have been required to perceive the tin it contained, except in exceptional instances, where it was found in little streaks, and bosses, and lumps. As to the reason of the tin being gathered together in the lowest portion of the deposit, cassiteris was more than several times the weight of common quartz sand, and sank rapidly, but its weight alone would not account for the fact. The tin was found in some ancient stream bed — not the same breadth the stream now occupied, as in the course of ages the stream would have travelled over a wide valley. The sand it brought down obstructed its own course, and caused it to constantly change its channel, and it was carried backwards and forwards across the valley and the deposit was washed much in the way it was done by artificial means. Every time the stream changed its course it washed the sand from the alluvial deposits, and turned it over and over again, allowing the water to pass through the midst of it, so that the tin was gradually left, and the sand, or lighter portions were carried away. It was in this way that the tin came to occupy the lowest portion of the deposit. It was covered with a deposit of alluvium sometimes as much as 30 feet in thickness, in others as much as 50 feet, but usually much less, sometimes as little as ten feet. The thickness or richness of the tin deposit bore no reference to the thickness of the deposit above it. It was merely a matter of chance, only he thought it might be inferred that where there was a thick deposit of alluvium over the tin, and the tin was not very thick, the deposit could not be of a very rich character. The audience would now understand how stream tin occurred.

What occurred in Perak occurred also in Australia, and in all the mines he had visited where stream tin was worked. There seemed to be an impression that the stream deposits in Perak were poorer than those in other places, whereas the fact was that if anything they were richer. If people thought, too, there was a larger deposit of sand over the tin, that was a mistake. It would be found that in some cases there was more in Cornwall, and it was so also in Australia. Probably in Australia the tin deposits had not been fairly worked, for on account of labour being so very dear none but the richest mines were worked, and these were worked very differently from the mines in Perak. People asked him, again, how it was that the result of tin mining in the Malay Peninsula was so unsatisfactory if the mines were as rich or richer than those in other parts of the world. That depended upon economical considerations with which he could have nothing to do, but still he could form his ideas upon the subject, which might be right or might be wrong, for clergymen were not as a rule very good men of business, and when he told his hearers what he thought about the matter at the termination of his lecture, they might take his remarks for what they were worth.

Now they might consider what granite was. Its principal ingredients were mica, feldspar, and quartz, but granite was very rarely so simply composed as that, and in common use the name applied to a dozen different kinds of rocks having diverse chemical compositions. In his opinion, and in that he was following the ideas of most of the leading geologists at home, granite was formerly a stratified rock, having assumed its present form after having been subjected to great pressure. Pressure generated heat, and with steam that was confined — of course having

no means of escape under the pressure to which the granite was subjected — produced a force which no rock was able to withstand. Superheated steam was more corrosive than any acid; they had no knowledge of any acid which melted away quartz but one, and that only to a small degree. Rocks by this process, however, were reduced to a pasty condition. There was one fact about tin deposits in Perak to be noted, and that was that they were never found except at the junction of Palæozoic rocks and granite. The Palæozoic rocks were the most ancient stratified rocks with which they were acquainted. Now these Palæozoic rocks lay on the top of the granite, and if the junction between the two was followed, it would be found that there was a gradual passage from one form to the other. Stratified rocks bore marks of stratification like sand-stone or slate, and as they were followed down it would be noticed that these marks became less and less distinct, while crystalline structure became more and more distinct until gradually there was a passage between these Palæozoic rocks into granite, so that they appeared as if the one was derived from the other. That, however, was a matter of theory.

The fact they found was that where they found minerals was always at the junction of granite or intrusive rock with a stratified rock. There was no exception to this rule throughout mining in the Malay Peninsula; he had never seen any tin deposits except such as were derived from the wearing down of the rock which were at the junction of the slates with the granite. If he were asked why that was so, he could give them a reply which was satisfactory to himself as the most reasonable way of accounting for it. All rocks were derived from marine action, and water, no matter how pure it might be, contained in solution some proportion of all the metals. Sea water, for instance, contained an appreciable amount of gold as well as silver. Supposing these rocks had been thrown down or sunk beneath the earth's surface, and subjected to the influence of heat generated from the rocks being piled above them, the metals would run together in consequence of the rocks being reduced to the pasty description which he had described, even when the particles were so finely divided as would be the case of gold in sea water. The quantity, they might say, was infinitesimally small, but the amount of gold taken out of the earth as compared to the amount of rock which was crushed and beaten for gold was infinitesimally small — much less than the quantity in the sea water at the present time. In the first state these rocks contained a certain amount of tin, and in the course of time the rocks became transmuted by the action of the steam he had described, and the result was that the tin became aggregated into fine grains and was more or less unequally distributed throughout the granite. But why was tin found at the junction of the Palæozoic rocks and the granite? Possibly it was by being brought into a state something like sublimation, that was converting the metal into vapour, from which state it would be condensed on the cooler or untransmuted portions of the stratified rock. In giving that explanation, he was aware that the term sublimation was not strictly correct, but he used that because it would convey to his hearers the nearest idea of what he wished to explain to them. Certainly, they found that the tin existed most in the uninterrupted parts of the Palæozoic rocks. That was a fact which he was glad to say was being more recognised than previously.

Over and over again he had pointed out to those searching for minerals in Perak that there were two things they must look for, first, a place where there had been a watercourse, and, second, a place where the rock had been composed of a junction of the Palæozoic rock with the granite. There was no rule of thumb for finding tin. He knew that there were some who would say, "That looks like a likely place, put down the rod and you will find metal." Sometimes they were right, but more often they were wrong and it was a mere chance if they were ever right. This was mere charlatanism which practical business men should put aside as much as possible. He thought there had been a little of that in the Malay Peninsula. In those cases the simple rule was lost sight of that alluvial tin could only be expected where there had been a watercourse, and, secondly, that not only must they look for it at a watercourse but also in those places where the rocks had been composed of the junction of the Palæozoic rocks and the granite. Now how was that to be told? Generally, they could tell it by the colour of the soil, but not always. The soil ought to be a reddish or yellowish colour, because the peroxide of iron in the Palæozoic rocks generally gave rise to a fiery red soil. If these rules were adopted, it was easy to tell whether any place was a likely place to find alluvial tin in, but they must not go so far as to say that as a matter of course tin was to be found there, though nearly always such was the case.

Then another thing was to be said about the deposits of tin. They were not to be worked on ordinary mining principles, because the alluvial soil was at most only some fifty feet thick, rarely so much, and more often only fifteen or sixteen feet. Under these conditions, they could not go tunnelling and burrowing and putting in operation all those mining apparatus found so useful where great depths below the surface had to be reached. Practically they could do nothing but strip off the alluvial soil, and on coming to the tin extract from it the deposit by means of washing. That was the simple process of mining in the Malay Peninsula. He did not know of any other place where similar deposits could be worked otherwise unless they were very deep, and then they could not be worked profitably unless they were unusually rich. He did not think that had been recognised by the Europeans concerned in the Malay Peninsula. It had been assumed they could sink shafts and run galleries and tunnels. That was sure to lead to failure. Even if they could do so they were sure to meet with large granite boulders, and, as they could not use dynamite where the working was so shallow, they would have to work round the rocks and leave a good deal of valuable tin in doing so. No doubt it was very gratifying to our national vanity to be able to show the Chinese what could be done with machine appliances, and if it were merely a benevolent undertaking that would be very interesting, but in practical work it led to a great deal of unnecessary expenditure and could not result in any great gain to those interested. His impression was that the mistake had been in not mining as the Chinese do. They strip off the soil in the most economical manner, they wash their tin deposits without any great appliances, and in that way they make their money. Probably it would be said the Chinese have advantages which we do not possess. This was perfectly true, and it seemed to him, if he might offer a suggestion — and this was where he meant his ability was not sufficiently trustworthy — he would suggest that the mines should

be carried on by Europeans on the tribute system rather than by the use of appliances which from the nature of the case could not be expected to pay.

He would give an illustration of this. In the early days of gold mining in Port Philip some of the mines gave very large dividends. One of the most prosperous for a time was the "Clunes" mine,[1] but as they went deeper and deeper the quartz got poorer and poorer until at last it was found that the mine did not pay. The shareholders got alarmed, and some of them were for selling off and realising their property as soon as possible. However, better counsels prevailed. Those who understood the matter said — "Look here, these miners are bringing up all sorts of stuff for you to crush in your mills. They do it because they are paid so much a ton, and they will go on digging without reference to anything but bringing so many tons to grass." The miners were then told they could take so many cubic yards, they could take all the gold and the company would pump and wind for them. The consequence was that the Clunes mine, instead of becoming a loss, was soon returning nearly as large dividends as it did previously. He thought the only way to mine successfully in Perak would be on a somewhat similar principle. Let the tributaries be the Chinese, who knew how to work the mines, and let as few Europeans as possible have anything to do with it. The climate was bad, the privations great, and the Europeans employed deserved to be paid good salaries; but let there be as few such as possible.

He would not detain them further. He could say more, but what he had said he hoped was sufficient to make clear to them that mines in Perak are no exception to mines elsewhere. They are rich; they offer favourable fields for the employment of British capital; but they require care in the working. In the end, they cannot fail to yield satisfactory results. [Applause]. He begged to thank the audience for the attention with which they had listened to him, and His Excellency for presiding. — The lecturer on resuming his seat was loudly applauded.

Mr. E. J. ACKROYD — Ladies and gentlemen, I am sure I do but express the sentiments of you all when I rise to move a vote of thanks to the Rev. Mr. Tenison-Woods for the very clear and lucid lecture he has given me to-day on the tin deposits and tin mines of Perak. [Applause.] His great knowledge of the subject and his experience have enabled him to treat it in such a simple and clear way that he has made it intelligible to many who were not acquainted with geological terms. To those who have no interest in these mines but came for the love of science I am sure he has afforded great pleasure, while to those who are interested he has given valuable information which may lead to practical measures being taken for the proper working of the mines. I beg to tender Mr. Tenison-Woods our best thanks for the very pleasant afternoon he has afforded us. [Loud applause.]

[1] Henry Alderson Thompson, Description of the Clunes Gold Mine, Victoria, *The Sydney Magazine of Science and Art* 2: 79–80, 1859. (Paper presented at the Philosophical Society of N.S.W. on Aug. 11, 1858.) https://archive.org/stream/sydneymagazines01socigoog#page/n95/mode/1up

THE GOVERNOR — I offer you, Mr. Tenison-Woods, the thanks of the meeting tendered by acclamation.

Rev. TENISON-WOODS acknowledged the compliment and moved a vote of thanks to His Excellency for so kindly presiding. Carried by acclamation.

THE GOVERNOR, in acknowledging the compliment, said that, as Mr. Tenison-Woods had said, when he was in Australia he was always ready to promote geographical or geological explorations. He assured them he should always be the same so long as he remained in Hong Kong.

Cover Note by Governor George Bowen

On the day following the lecture, Governor Bowen sent a copy of the newspaper report to the Colonial Office in London:

Government House,
Hong Kong, 4th February, 1885
The Right Honourable The Earl of Derby KG

My Lord,

I have the honour to report that I was yesterday requested to take the chair at a lecture delivered by the Reverend Julian Tenison-Woods on "The Mines and Minerals of the Malay Peninsula." As I said in the speech with which I opened the proceedings, I had known Mr. Tenison-Woods for twenty years while I was Governor successively of Queensland, New Zealand, and Victoria, as an eminent geologist and Mineralogist, and as the author of one of the best books on the exploration of Australia. He has lately explored the mining districts of the Malay Peninsula, under the auspices of the Governor, Sir Frederick Weld. His lecture (of which a report is enclosed) was very well received and (as I observed) was of practical and financial as well as scientific interest to many person in this community who have advanced funds for the working of the Mines in the Malay Peninsula.

I have the honour to be, My Lord, Your Lordship's Most Obedient, Humble Servant, G. F. Bowen

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Ronald N. Bracewell: an appreciation

A. Richard Thompson* and Robert H. Frater**

*National Radio Astronomy Observatory, Charlottesville, VA, USA
**Resmed Ltd, Elizabeth Macarthur Drive, Bella Vista, NSW, Australia

Email: athompso@nrao.edu
Email: BobF@resmed.com.au

Abstract

Ronald Newbold Bracewell (1921–2007) made fundamental contributions to the development of radio astronomy in the areas of interferometry, signal processing, and imaging, and also to tomography, various areas of data analysis, and the understanding of Fourier transforms. He was born in Sydney, Australia, and received a B.Sc. degree in mathematics and physics, and B.E. and M.E. degrees in electrical engineering from the University of Sydney, and his Ph.D. from the University of Cambridge, U.K., for research on the ionosphere. In 1949 he joined the Radiophysics Laboratory of CSIRO, where he became interested in radio astronomy. In 1955 he moved to Stanford University, California, where he became Lewis M. Terman Professor of Electrical Engineering. He retired from teaching in 1991, but continued to be active in radio astronomy and other applications of imaging techniques, etc. During his career he published ten books and more than 250 papers. Honors that he received include the Duddell Premium of the Institute of Electrical Engineers, London, the Hertz Medal of the IEEE, and the Order of Australia. For his work on imaging in tomography he was elected to Associate Membership of the Institute of Medicine of the U.S. National Academy of Sciences.

This is a version of Thompson and Frater (2010), edited by Robert Marks. Reprinted with permission.

Keywords: Ronald N. Bracewell, radio astronomy, CSIRO Division of Radiophysics, Stanford University

Education and Early Years

Bracewell attended the Sydney Boys High School 1933–1937. In addition to science and mathematics he was interested in languages and passed oral exams in French and German. In 1937, he was awarded the Alliance Française Prize and came third in the state in French. After high school, he was a student at the University of Sydney and in 1941 graduated with a degree in physics and mathematics, and in 1943 with a degree in Electrical Engineering with First Class Honours. He also received an M.E. degree in 1948 for his work at CSIRO. He spoke of the influence of Oxford-trained Victor A. Bailey in his approach to physics and of Joseph L. Pawsey, a student of Cambridge Professor John A. Ratcliffe, in the duality of his physical versus mathematical approach to transmission lines and antennas.

In 1943–1945, during WWII, he was a member of the Radiophysics Laboratory of the CSIR (from 1949, CSIRO, Commonwealth Scientific and Industrial Research Organization), in Sydney, and worked with Pawsey and Edward G. ('Taffy') Bowen on development of radar and radio communications. In 1946, after the War, Bracewell went to Sidney Sussex College at Cambridge (England) as Ratcliffe's graduate student. His research topic was the study of the iono-

sphere using propagation measurements at 16 kHz, for which he obtained his Ph.D. The ionospheric work resulted in the discovery that the D layer ionization consists of two components, for which he was awarded the Duddell Premium of the Institute of Electrical Engineers in 1952. The effect of solar activity on the ionosphere was one of the factors that led to Bracewell's lifelong interest in the Sun. His interest in the theory and applications of Fourier transforms, which was initiated by mathematics courses at Sydney University, was further stimulated by Ratcliffe who was a recognized authority on the subject.

Introduction to Radio Astronomy

In 1949 Bracewell returned to Australia and again took up a position at CSIRO. Initially, he continued his work on the ionosphere, but soon became involved in radio astronomy which was being actively pursued. He shared an office with radio astronomers W. N. ('Chris') Christiansen and Harry Minnett, who were working on solar radio observations (see Orchiston, Slee and Burman, 2006). Christiansen had built grating arrays of parabolic antennas, aligned in N-S and E-W directions, along the edges of a reservoir at Potts Hill near Sydney (see Wendt et al., 2008). These produced fan-beam scans of the Sun over a range of angles each day. From these it was possible to derive radio brightness contours of the Sun in two dimensions (see Christiansen and Warburton, 1955a; 1955b). Bracewell was interested in this analysis, which involved Fourier transforms. He was also intrigued by the possibility of using two grating arrays to produce a matrix of pencil beams, using the cross configuration developed by Bernie Mills for linear arrays (see Mills and Little, 1953). During this time Pawsey, the leader of the radio astronomy group, invited Bracewell to be co-author of the book *Radio Astronomy* (Pawsey and Bracewell, 1955), and Bracewell later surmised that this was partly a device to get him more involved in the subject. Pawsey also asked him to produce a pictorial dictionary of Fourier transforms, which later led to Bracewell's most important book, *The Fourier Transform and its Applications* (Bracewell, 1965).

During the academic year 1954–1955, Bracewell was invited by Otto Struve to give a series of lectures on radio astronomy at the University of California, Berkeley. He also lectured at Stanford University, which led to his joining the Electrical Engineering Department at Stanford in December 1955. An interesting autobiographical account of the period from his first interest in radio astronomy through the early years at Stanford can be found in his paper, "Early work on imaging theory in radio astronomy", published in Sullivan's *The Early Years of Radio Astronomy* (1984), while his recollections of the Stanford years can be found in Bracewell (2005).

Important Early Papers and a Book

During the period 1949–1965, from his first interest in radio astronomy through his early years at Stanford, Bracewell produced a number of publications on interferometer theory, imaging with interferometers and arrays, and data analysis that established his expertise in this area. Examples of notable publications are discussed below.

"Aerial Smoothing in Radio Astronomy" (Bracewell and Roberts, 1954)

This paper was particularly important in the early years of radio astronomy, when the relation between the true profile of a source and the profile obtained by scan-

ning with an antenna was not well understood. Bracewell and James A. (Jim) Roberts explained the scanning as a convolution of the brightness function and the point-source response of the antenna. The convolution theorem of Fourier transforms shows that the Fourier components of the source profile are filtered by the Fourier spectrum of the antenna response. The concept of invisible distributions (i.e. Fourier components not detectable with a given aperture distribution) was introduced in this paper. This provided essential insights into the observing process. The later part of the paper is concerned with reducing the effect of aerial smoothing by analytically adjusting the antenna response so that all of the Fourier spatial components to which it responds are given equal weight. Bracewell referred to this process as *restoration* and the resulting profile as the *principal response*. The angular resolution is usually improved by the restoration process, but the sharp cutoff in angular frequency at the maximum to which the antenna system responds can result in extensive sidelobes which limit the dynamic range.

"Strip Integration in Radio Astronomy" (Bracewell, 1956)

This paper considered the construction of two-dimensional images from one-dimensional scans of a source with a range of position angles as was required, for example, to obtain a solar map from Christiansen's early grating-array observations. The Fourier transform relationships involved are succinctly illustrated in a diagram (Figure 5 in his paper) which Bracewell later refers to in his chapter in Sullivan's 1984 book as the 'projection-slice theorem'. He used the term *reconstruction* to describe this method of production of two-dimensional images, a technique which was later adapted to tomography. Further development can be found in "Inversion of fan beam scans in radio astronomy" (Bracewell and Riddle, 1967).

"Radio Interferometry of Discrete Sources" (Bracewell, 1958)

This paper provides a precise development of the interferometer response and the Fourier transform relationship between the fringe visibility and the brightness distribution. The paper also unifies material discussed in earlier publications by various authors. Bracewell introduces the use of direction cosines for the angular coordinates on the sky, thereby avoiding the small-angle approximation used in most of the earlier discussions of interferometry. This paper also uses the sampling theorem of Fourier transforms to determine the most efficient choice of the spacings of antennas in interferometry.

"Tolerance Theory of Large Antennas" (Bracewell, 1961)

This paper precedes by several years the famous paper on the same subject by J. Ruze (1966). It is difficult to be sure about precedence of ideas on this subject, which was developing during the 1950s. Bracewell's paper is one of the earliest detailed analyses.

The Fourier Transform and its Applications (Bracewell, 1965)

The end of this period saw the publication of Bracewell's most important book, *The Fourier Transform and its Applications*. This book cemented the early 'physical versus mathematical' learnings from Pawsey, and the period with Ratcliffe, in a book that became the Fourier transform 'bible' for many in

the radio astronomy scene.[1] Through it, Bracewell established a level of recognition for his elucidation of the convolution theorem and its importance in the interpretation of observations. He developed a reputation for his demanding exactitude from those who worked in his space.

Stanford and the Heliopolis Observatory

At Stanford, Bracewell established a Radio Astronomy Institute and also an observatory, which he named Heliopolis. This was located on the outskirts of the Stanford lands. The first instrument developed at Heliopolis was a solar cross, i.e. a crossed-grating array for solar observations, as he had considered earlier while at CSIRO. This array was made to Bracewell's design, and consisted of 32 parabolic antennas arranged in two linear arrays and operated at 9.1 cm wavelength. It is described by Bracewell and Swarup (1961). Phase adjustment of the cross led to the invention of the round-trip phase measurement technique by Bracewell's graduate student Govind Swarup,which is described by Swarup and Yang (1961). An adaptation of this round-trip technique has subsequently been used in almost all large radio astronomy arrays. The solar cross was used to make daily maps of the Sun, with an angular resolution of 3.2 arcmin from June 1962 to August 1973.[2] These were published monthly, and the observations also resulted in a number of papers on radio emission from the solar corona. Two additional antennas were added to extend the east-west arm of the cross to form a compound interferometer. This produced fan beams of width 52 arcsec, and east-west scans of several strong radio sources were obtained with this angular resolution (Swarup, Thompson, and Bracewell, 1963; Thompson and Krishnan, 1965).

The east-west arm[3] of the cross array was also used to study the radio source Centaurus A, a radio galaxy that was strong, optically identified, and of sufficient angular width that the beam of the arm could reveal interesting structural detail. The two components of the central part of the source were detected (Little, Cudaback, and Bracewell, 1964). In April 1962, during a trip to Australia, Bracewell had the opportunity to observe Centaurus A with the Parkes Radio Telescope at 10 cm. He was able to resolve the two components in the central part by driving the telescope in both azimuth and elevation simultaneously, so as to scan in the direction of the component separation. He was also able to rotate the feed and discover the linear polarization. However, there appeared to be some question of whether the observation was made during an officially-granted observing time, and Bracewell's letter

[1] Petrosian (2009) recounts that "before an observing run at Kitt Peak, I needed to refer to this book. I looked for it in the shelves of the library at the National Optical Astronomical Observatory in Tucson, Arizona, but could not find it. The librarian informed me that the book had been signed out. I told her that this [was] a very useful book, and they should have more than one copy. She agreed and said that there were indeed eleven copies; all were in use by the resident astronomers."

[2] Petrosian (2009) remarks that this "provided daily maps of the Sun for more than a decade encompassing more than one solar activity cycle of eleven years. These maps were useful in predicting magnetic storms caused by solar activity and were used by NASA during the first landing on the Moon."

[3] The maximum elevation of Centaurus A was too low for useful resolution with the north-south arm. For observations of Centaurus A, a parametric amplifier developed by A.G. Little (1961) was used to improve the sensitivity. Alec Little obtained an M.S. degree from Stanford for his amplifier work.

to *Nature* (Bracewell, Cooper, and Cousins, 1962) was not published until 29 September 1962. Meanwhile, other observations made shortly after Bracewell's, also reporting polarization of Centaurus A, appeared in print a few weeks before Bracewell's letter. More detailed accounts of these circumstances can be found in Bracewell (2002) and Haynes et al. (1996).

At Heliopolis, there were also two 30-ft diameter equatorially-mounted parabolic antennas, and during the 1960s these were used as a two-element interferometer at 9.8 cm (~3.1 GHz). They could be moved between several foundations to vary the length and direction of the baseline. The interferometer provided material for several of the Ph.D. theses listed in Section 8, but the collecting area was too small for observation of more than a few of the strongest galactic and extragalactic sources. Bracewell considered building an instrument with a much larger collecting area, using several long cylindrical reflectors. He envisaged an instrument that would grow with time, by the addition of more elements as funding allowed (Bracewell, Swarup, and Seeger, 1962). However, funds for a large instrument proved to be unavailable, and the development of Earth-rotation synthesis by Martin Ryle showed the advantage of fully steerable antennas. Thus, Bracewell concluded that the most economical way to obtain sensitivity would be by building an array of tracking antennas which could be designed and constructed under his direction. This resulted in five 18.3-m (60-ft) diameter antennas, which were made to Bracewell's design and constructed on-site at Heliopolis. The antennas were configured as an east-west, minimum-redundancy, linear array devised by Bracewell (1966), in which all spacings up to nine times the unit spacing are included. The operating frequency was 10.7 GHz, allowing synthesis of a beam of width 18.8". A well-illustrated description of the construction project is given in Bracewell et al. (1971) and full details of the array in Bracewell et al. (1973). Observations with the array provided data for a number of papers and theses by Bracewell's students, including further work on Centaurus A (Price and Stull, 1973). This array was in operation from 1972 until the closing of the Heliopolis observatory in 1979.

The discovery in 1964 of the cosmic background radiation (CMB) by Arno Penzias and Robert Wilson (see Penzias and Wilson, 1965) provided a radio astronomical feature that could be investigated without the use of large antennas. Bracewell realized that although the measurements made in the early years after the discovery indicated a uniform brightness temperature, the motion of the Earth with respect to the CMB would cause an observable variation, which he and his graduate student E.K. Conklin were able to calculate (Bracewell and Conklin, 1968). From observations at Heliopolis, only upper limits on the variation could be obtained. To reduce atmospheric absorption, the project was moved to a high-elevation site in the White Mountains of California, using two small horn antennas at a frequency of 8 GHz. Conklin (1969) was then able to publish a determination of the velocity of the Earth from measurements of variation of the observed CMB temperature at the mK level. This was the first detection of the effect, and a notable achievement considering that it was made with a simple system using two small horn antennas with an uncooled receiver.

In the late 1960s, Bracewell's work on reconstruction of images from one-dimensional scans became recognized as having an important application in medical imaging by X-ray tomography. As mentioned above, the theory of reconstruction of a two-dimensional image from one-dimensional scans had been explained by Bracewell (1956). The implementation was further advanced in the paper with graduate student A.C. Riddle on "Inversion of fan-beam scans in radio astronomy" (Bracewell and Riddle, 1967). In this later paper, the procedure is simplified by the avoidance of the need to compute Fourier transforms. Bracewell wrote two further papers specifically on tomography, one with graduate student J. Verley (Verley and Bracewell, 1979). He also devoted a chapter on tomography in his book *Two-Dimensional Imaging* (Bracewell, 1995). For his contribution to tomography, Bracewell was awarded associate membership of the Institute of Medicine of the U.S. National Academy of Sciences in 1962, the first Australian to achieve this distinction.

Work at Stanford after Heliopolis

Funding for the operation of Heliopolis was discontinued in 1979, as a result of the general policy of supporting a single national observatory for radio astronomy rather than a number of smaller ones operated by individual universities. Thus, radio astronomical observations at Stanford were discontinued, but Bracewell's interest in radio astronomy and related sciences continued unabated. In Bracewell (1978) he suggested the use of interferometry in space for detection of non-solar planets, and this idea and further details are discussed in several later papers.[4] These describe a proposed application of infrared interferometry using space vehicles, in which a null in the fringe pattern is steered onto the position of a star to allow a search for much fainter images of planets. The idea has been widely discussed as a possibility for terrestrial telescopes or a future space mission (see, e.g., Hinz et al., 1998).

Starting in 1983, Bracewell published 13 papers (e.g. Bracewell 1984b) and a book (Bracewell 1986) on mathematical development of the transform introduced by Hartley, which is similar to the Fourier transform but does not involve complex factors. He developed Hartley versions of the numerous theorems and relationships that are well known in Fourier transform theory, and also a fast Hartley transform (FHT) algorithm, which could in many cases be used as an alternative to the fast Fourier transform (FFT). The avoidance of complex quantities for transformation of real data in the FHT allowed it to perform twice as fast as the versions of the FFT in use at the time, but later improvements to the FFT overcame its disadvantage in speed.

During the period 1985–1989 Bracewell published a number of papers on sunspot statistics and the solar cycle. In a much earlier paper (Bracewell, 1953) he had pointed out that since the magnetic polarization of sunspots changes sign in alternate 11-year sunspot cycles, the sign of the sunspot number should be reversed in alternate 11-year cycles, revealing a 22-year periodicity. This applies to studies in which the

[4] On May 16, 1978, Bracewell gave the Pollock Memorial Lecture at the Royal Society of NSW, outlining his ideas for detecting life in outer space (Bracewell 1979).

sunspot number is used as a measure of the active nature of the Sun. Without this sign reversal, the oscillations of the 22-year sunspot function are effectively rectified, and artificial frequency components can be introduced. This important derectification step was included in Bracewell's later work in the 1980s. Examination of the sunspot numbers when plotted with the derectified 22-year cycle led him to the discovery of a three-halves power-law in the annual mean values, which he considered to be one of the more important results of his analysis (Bracewell, 1988a). He was interested in the basic mechanism of the sunspot cycle, and in Bracewell (1989) discussed a possible theory in which magneto-mechanical waves propagate outward from a source at the center of the Sun.

Another sunspot-number feature that Bracewell investigated involved a possible relationship with a series of geological laminae from the late Precambrian era, located in the Elatina area of South Australia, and hypothetically identified as varves (Bracewell, 1988b).[5] These had been studied by a geologist (Williams, 1985) who suggested that periodic variations in the thickness of the layers could be interpreted as indicating a time scale similar to that of the sunspot numbers. A putative mechanism linking the structure to solar radiation involved flow of melt-water and resulting variations in water levels on depositions in a lake. Thus, hypothetically, the thickness of the layers could provide an indication of the variation in the strength of solar radiation from year to year. However, a similar layered structure was later found in a different region of South Australia (Sonett et al., 1988) in which the geological situation suggested a luni-solar tidal mechanism rather than a solar radiation mechanism. The tidal mechanism was also found to be applicable to the Elatina laminae, and the solar cycle interpretation of the Elatina data has been largely abandoned.

Bracewell as a Teacher and Mentor

Bracewell always presented his lectures with an infectious enthusiasm. He was a challenging taskmaster for his graduate students. Bracewell had a good eye for, and an appreciation of, capable people and delighted in being able to stretch their capabilities. He was a great mentor and always demanded clear and detailed thinking when approaching any problem. He insisted upon precise definitions and disliked making changes in them. Thus, in his papers on interferometry he preferred to follow Michelson's original definition of fringe visibility, in which the zero-spacing value is normalized to unity, rather than expressing it in units of flux density as has become the common practice. He never fully approved of the use of image processing routines that introduced Fourier components of the brightness that had not been measured in the observations. His careful understanding of basic concepts and detailed thinking enabled him to make contributions in many fields, and influenced the lives of students and colleagues who worked with him.

When examining students, Bracewell liked to try to judge their ingenuity and power of observation as an indication of aptitude for experimental research. During

[5] The Elatina layers were measured in detail from core samples. In terms of the solar cycle interpretation they covered a range of more than 1,300 years and thus might have provided a major chronological increase in solar-cycle data. Note that the term "varves" applies specifically to layers deposited in annual cycles.

annual interviews with prospective Ph.D. students in the Department of Electrical Engineering, he often tested their reaction to unusual things that they had not seen before.[6] One year he asked each student to examine a piece of wood that he had made. This was approximately boat-shaped and had the property that when set spinning in either a clockwise or counter-clockwise direction, it ended turning in the clockwise direction.[7] He wanted to see a careful inspection of the object, and tests of how it behaved under various conditions, rather than an attempt at a mathematical exposition. In another year he asked the students to examine a sample piece of the circular waveguide used for signal transmission in the VLA, without telling them what it was. A careful visual examination of this would show that the surface impedance of the inner wall was very low in the circumferential direction, but much higher in the longitudinal direction, which could provide a clue as to its use.

Bracewell retired from teaching in 1991, but continued to work in his areas of interest. His list of publications from these later years contains 22 papers and 12 book reviews. In 1994 he was awarded the Heinrich Hertz Medal of the IEEE for pioneering work in antenna aperture synthesis and image reconstruction as applied to radio astronomy and to computer-assisted tomography. In 1998 he was named Officer of the Order of Australia for his service to science in the fields of radio astronomy and image reconstruction.

[6] For a list of his graduate students and their affiliations, see Section 8 of Thompson and Frater, 2010.

[7] The "rattleback" phenomenon (see, e.g., Walker, 1979). The examination in which the device was used was in 1977.

Breadth of Expertise and Interest

Bracewell's mathematical expertise is evident from much of his work, especially his books on the Fourier and Hartley transforms. He also had an excellent understanding of physics as is evident in publications such as "Rotation of artificial earth satellites" (Bracewell and Garriot, 1958) and "An observer moving in the 3° K radiation field" (Bracewell and Conklin, 1968). In Mihovilovic and Bracewell (1991) he and his student introduced the concept of chirplets as a representation for ionospheric whistler signals and similar data in a time-frequency domain. Practical engineering skills can be seen in Bracewell's designs of both the solar cross and the five-element array. In the latter, the detailed antenna design was his conception, and enabled the array to be implemented at relatively low cost. He enjoyed being involved at a hands-on level in engineering projects. An example of his understanding of fundamental theory in engineering is the paper on "Impulses concealed by singularities: transmission-line theory" (Bracewell, 1998).

The remarkably wide range of Bracewell's scientific interests can be clearly seen in the diversity of the subjects of his publications and lectures. Throughout his career he had a long-term interest in the possibility of the existence of extraterrestrial intelligence, and the practicality of extraterrestrial communication. This resulted in 19 papers and the book The *Galactic Club* (Bracewell, 1974). An example of his interest in the history of science and engineering can be seen in the paper "Planetary influences on electrical engineering" (Bracewell, 1992). He designed sundials, one of which was installed at the Terman Building on the Stanford campus. Bracewell had a life-long interest in trees,

particularly those native to Australia, and in California he identified more than seventy species of the introduced eucalypts. He wrote two books on trees of the Stanford area (see Bracewell, 2005) and had some fine examples of banksias growing in the garden of his house at Stanford.

Further Information

Some of Bracewell's own descriptions of his work can be found in his chapter in Sullivan (1984), Bracewell (2005), and the text of a recorded interview by Ragbir Bhathal FRSN on 10 June 2000, for the Oral History Section of the National Library of Australia.[8] Bracewell's scientific papers are archived at the National Radio Astronomy Observatory, Charlottesville, VA. A complete list of his publications can be found at http://www.nrao.edu/archives/Bracewell/bracewell_top.shtml. This list includes 10 books, 218 articles in the open literature, 33 book reviews, and 34 internal reports.

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[8] See Bracewell and Bhathal (2019).

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Professor Ronald Bracewell

Interviewed by Ragbir Bhathal

This is an edited version of an interview between Ronald N. Bracewell and Ragbir Bhathal FRSN that took place on the 10th June, 2000, in Sydney (TRC-4596). Ron Bracewell AO was born in Sydney on 21 July 1921. Irene Kelly provided further information.

Bhathal: What was your impression of Joseph Pawsey[1] and Edward Bowen[2] at the CSIRO Radiophysics Lab in 1942, during the war?

Bracewell: I worked very closely with Pawsey and ultimately we wrote a book together. He was an admirable leader. His knowledge of physics was intimate. He didn't do high-powered theory but he understood electromagnetic phenomena in a very intimate way. He'd had experience with the television transmitter at EMI in England and he really understood how electrical things worked. He gave a course of lectures shortly after I arrived on transmission line theory. I still have the notes. I believe that set of lectures deeply influenced many of the people that went through Radiophysics, gave them a feeling for how you think about electrical things, as distinct from starting with Maxwell's equations and trying to deduce everything from that. There's a different way of doing experimental electricity and Pawsey conveyed that very well.

He developed loyalty. He was a group leader who managed a number of ongoing activities very well. Bowen arrived somewhat later. Bowen was given the job of taking the first powerful magnetron across the Atlantic to the United States. When he'd finished that, he came on to Sydney. At that time there was some uncertainty about the management of the place. D. F. Martin had been the first chief of Radiophysics. He had been pulled out and gone to work on ionospheric things and we had an interim chief, John Britten, who was not a physicist but was certainly a manager — on the other hand, not quite appropriate — and for a time Fred White[3] ran the place. Bowen turned out to be the long-term resolution of this period of uncertainty.

We all got to know him very well, a dynamic person, good background in physics, great experience with radar which he'd been involved in since 1935, so he really knew the subject that we were dealing with, and quite original. He just didn't take orders from people, he thought of things. The Parkes dish is an example of an initiative that he took. As a trivial example was this: When we heard about jet aircraft, jet engines, Bowen began to build in the workshop at Radiophysics indoors a big propeller about two metres long and hollow, a metal propeller. This was welded together and fabricated, not an easy thing to make. It had a big hub and it was suspended on a couple of saw horses and it could spin, you see. It was unusual in that the ends of this propeller took a sharp turn at right angles at the tips, very short, and there was a small hole. The idea

[1] Joseph Pawsley FRS (1908–1962). http://adb.anu.edu.au/biography/pawsey-joseph-lade-joe-11353

[2] Edward Bowen FRS (1911–1991). http://adb.anu.edu.au/biography/bowen-edward-george-17857

[3] Sir Fred White FRS (1905–1994). http://adb.anu.edu.au/biography/white-sir-frederick-william-fred-1035

was you would now feed petrol in through the axle of this propeller under pressure and it would squirt out through the small holes at the end in opposite directions arranged like a Z, you see, and you would set light to this petrol and it would drive the propeller.

You would now have a jet propulsion device without all this nonsense of turbines and expensive blades and so on and so on. If you can imagine this and a workshop full of fellows all watching this explosive device and who's going to light the wick, as it were.

It actually ran. I saw it running. It was terribly dangerous and it never came to anything but that was the sort of initiative he took, and he also was very early at work on the notion of building antennas which would adjust their own shapes by a servo-mechanism. He had a small test of that kind. He was a very ingenious fellow.

I applied for the CSIRO studentship and Bowen and Pawsey would have supported that very strongly. Bowen's support in fact was so strong — and this illustrates a little bit about his personality too — when I got to Cambridge, J.A. Ratcliffe[4] welcomed me in his office and as I came in he was closing his desk drawer. He had clearly just been reading the letter of recommendation which Bowen sent, just to refresh himself, and he said he was delighted to have me there. He said, "I'm particularly pleased to know that you've had some experience with the ionosphere". I said, "No, I've never done anything with the ionosphere". A frown crossed his face and he opened his desk drawer, pulled out this piece of paper and read it very carefully and he kept frowning and put it back in the desk drawer. Bowen had clearly perjured himself to ensure that I got in. I couldn't complain about that but it's a revealing item on personality.

Bhathal: What was your impression of Ratcliffe as a scientist and a person?

Bracewell: Admirable. He was in a tradition of experimental physics in the Cavendish lab. He was well acquainted with J. J. Thompson[5] and other significant figures in other sorts of physics, but they were all very closely grouped in one building. He thought very logically, he thought intuitively, physically. He thought in terms of actual things. At the same time, he'd had at high school a very good basic training in mathematics. He could do mathematics but he laughed when you did mathematics to prove something when he could see it physically at a glance and he would explain to you, "This is the real reason, you see. It's got nothing to do with Bessel functions. It's because … " and then he would make it all clear to you.

He'd had experience during the war making electronic devices, so he had a good background and he was an extraordinarily good teacher. He could clarify things. We learnt a lot of good things from him like how you behave when visiting lecturers come and so on. Someone would come to our weekly seminar and give a talk and Ratcliffe would ask very difficult questions, you see, almost rude, in fact, the complete opposite to what happens in the United States where, if the lecturer gets up and gets it completely wrong, no-one will say a word, where in England they'd jump on you. In Australia they don't do it in quite such a sophisticated way but they're pretty rude here too, which you have to be prepared for.

[4] J. A. Ratcliff FRS (1902–1987).
https://en.wikipedia.org/wiki/J._A._Ratcliffe

[5] Sir J.J. Thompson FRS (1856–1940).
https://en.wikipedia.org/wiki/J._J._Thomson

Bhathal: What was your PhD about? Can you tell us about it? Did you find anything significant?

Bracewell: Well, I was given a choice of jobs. Ratcliffe had a list of things he wanted to do. I may have arrived a week or two before some of the others, so he said, "You can work on metre wavelength stuff on the ionosphere or you can work on very long waves," which were wavelengths of about 15 kilometres. I had been working on the short-wavelength fringe for four years starting at 20 centimetres and 10 centimetres and 3 centimetres and then my friends were working on just a few millimetres wavelengths, and I thought, "Well, I've been at the cutting edge of the spectrum of that end. I'll go to the other end." That was my logic, totally baseless, so I got on to the 16-kilohertz propagation. There was a transmitter at Rugby with several towers almost a thousand feet high which had been used, or intended, for communications through the Empire. You could pick that transmitter up in Australia all the way round the world, you see, which is due to the fact that there's reflection from the ionosphere and not all that much loss. Just how this propagation took place depended on properties of the ionosphere that were not then known.

I picked up on that. There had been some previous work before the war started, then that terminated, so partly I was trying to explain earlier observations and partly get new ones. Ratcliffe would arrange for the Rugby transmitter, GBR, to transmit at certain times of the day and night. Tom Straker and I would go to out-of-the-way places where you could plug in the electrical things and take antennas with us and make measurements manually all through the night. What we found, which is significant, is that the D region actually consisted of two regions, D alpha and D beta, and that one of these began moving in the early morning at sunrise as seen from a height of 80 kilometres. Twenty minutes or so later another region would be affected when the sun was on the horizon. We could see that there are two regions, the lower of which is transparent to the ionising radiation understood to be ultraviolet, and it would go through that region twice and come out and start the first region going and then later the second region would go. This hadn't been at all clear before and I think this was one of the significant things done.

The other thing we did was to follow up the sudden ionospheric disturbances which cause short-wave fade-outs and we found that's due to the D region being pushed down by ultraviolet radiation accompanying solar flares, and when that ionisation gets pushed down, we see it as a reflecting layer, since we're using the long wavelengths, but to short waves all of a sudden you have ionisation in a region where previously there hadn't been any, so that causes the absorption. We did a lot of work on that too and that was probably the beginning of my interest in solar phenomena, because I'm supposed to be studying the ionosphere but as a matter of fact we've got a tool for studying solar phenomena also.

Bhathal: After returning from Cambridge, you continued at the CSIRO Radiophysics Lab from 1949 to 1954. What were the problems you were trying to solve at this time?

Bracewell: Well, I started working on a long-wave transmitter at Belconnen near Canberra, known as VHP, and began to make observations with Keith Bigg for two or three years. We travelled around New South Wales

having a wonderful time and learning a lot about geography. That was the first sort of thing that I did. Radioastronomy was going on around me, as it had been in Cambridge. Martin Ryle's[6] group was just in the next lab to mine, so I was thoroughly familiar with all that, and here I'm in a radioastronomy environment again but I'm still doing very long-wave studies on the ionosphere.

Bhathal: Let's talk about your period between '49 and '54. How did you get interested in radioastronomy, because you were doing more ionospheric work?

Bracewell: Well, I was just in very close contact with them, you see. It was going on around me and I just watched what was going on. In Cambridge I was looking at it pretty closely and I would get letters from Sydney asking me what Ryle was doing. Ryle was a very secretive person.

Bhathal: So you were sort of a spy there?

Bracewell: It would seem that there are people who thought this. Well, Martin Ryle did. He had a fear of persecution and he had instructed his co-workers that when people came from Manchester to pay a visit that everything had to be removed from the desks and put in the drawers. He was really — there's a word for it — paranoid. Ruby Payne-Scott[7] wrote to me and said, "We are getting these bursts from the sun of very short duration, just a few seconds. Ask Ryle if he's getting them." They were just short blips and could be interference. So I asked Martin, "Are you getting these things?". He said, "Oh, yes, we see them all the time but they're due to aeroplanes going over. The difference between the Sydney people, those colonials, and us is that we have a loudspeaker attached to our receiver so we can hear what's happening. Whenever we see one of those, we can hear a plane going over."

I wrote back to Ruby and she said, "Yes, I've heard that but we're convinced they're coming from the sun. Could you get me some records?" I went and asked Martin if I could trace some records. All the records were four-inch wide paper tape and there's reams and reams of this stuff. I asked him if I could make some tracings. He didn't have any objection to that, so I spent half a day there. Everyone could see what I was doing. I sent these to Ruby and she said, "That's exactly what we get and that's due to the sun." When I got back to Sydney Ruby produced a letter from Martin Ryle saying, "If I'd known that Ron Bracewell was spying on us … "

Then in Sydney I was also very close … you see, for quite a while I was sharing a room with Chris Christiansen[8] and Harry Minnett[9]. Christiansen was involved in up-to-date observations. Harry Minnett was beginning to work on plans for the 210-foot dish, but he had done radioastronomical observations on the moon that I was thoroughly familiar with and had written it up in our textbook. Christiansen was studying the sun with his 32-element array at Potts Hill and the observations he was getting were being analysed in the next room, among other

[6] Sir Martin Ryle FRS (1918–1984). https://en.wikipedia.org/wiki/Martin_Ryle

[7] Ruby Payne-Scott (1912–1981). http://adb.anu.edu.au/biography/payne-scott-ruby-violet-15036 See also: Halleck, Rebecca (2018), Overlooked No More: Ruby Payne-Scott, Who Explored Space With Radio Waves, *New York Times*, 29 August. https://www.nytimes.com/2018/08/29/obituaries/ruby-payne-scott-overlooked.html

[8] Chris Christiansen (1913–2007). https://csiropedia.csiro.au/christiansen-wilbur-norman/

[9] Harry Minnett (1917–2003). https://csiropedia.csiro.au/minnett-harry-clive/

people by Govind Swarup[10], who came and spent several years with me ultimately at Stanford. I knew all this radioastronomy was going on in my immediate vicinity. Then Pawsey had asked me to write the textbook with him, so I became pretty familiar with all branches of radioastronomy, whether it was in the next room or not.

It was a very interesting experience writing that book. Pawsey had listed the chapters, then he would write a draft. His way of writing was to write very fast, very large writing, and get it typed up and then he would scribble all over it and get it typed up again and go through an infinite number of revisions; this is great if you have the power over typists, and we had a room full of typists in those days. My method of writing was to think very carefully so that I didn't have to rewrite the same sentence twice. If Pawsey wanted to make a correction and say this is black or white and he changed his mind, he'd cross that out and he'd write down, "This is white". My method would be to cross out the "black" and the "or" and minimise the amount of ink wasted.

This is very interesting. It was like a sort of feedback system. He would write it and then I'd rewrite it and then it would get typed and then he'd scribble all over it and he would leave great blanks and he said, "This reason for this is … ", and then there'd be a great blank and I'd go and fill that in in the library. That was a good experience and that's how I got into radioastronomy. I've sometimes wondered, though I don't know for sure, whether this might have been resented by some of the working radioastronomers. On the other hand, they had all had the invitation previously and they felt, and quite rightly, I can understand, that making the next observations and beating the competition to the next discovery is much better than doing something historical.

But for me Pawsey actually manœuvred me out of ionospheric things. He judged that there was very little future there. The first thing he did was in 1952 to put me on the organising committee for the URSI[11] meeting in Sydney, the first scientific radio union to meet in Australia, and that kept me occupied for many months. The committee consisted of Madsen[12], Pawsey, Bowen, Piddington[13], another senior person whose name escapes me, and I was the so-called organising secretary. These fellows would all sit around and they'd figure out what was to be done next, then they'd look at me. The good side of this was I had infinite resources. I could go to other people in the lab and say, "I have been told that you are now going to do this." That took me out of action for at least six months. Pawsey had looked for co-authors and hadn't had any success, so he got me, you see, and I just had to study all these things up and write it, so I really just slipped into it.

There was a great paper by [Lindsay] McCready, Pawsey and Payne-Scott[14] in which they observed the sun. The problem was to find out where on the sun the very strong radiation was coming from. The

[10] Govind Swarup FRS (1929–). https://en.wikipedia.org/wiki/Govind_Swarup

[11] The International Union of Radio Science https://en.wikipedia.org/wiki/International_Union_of_Radio_Science

[12] Sir John Madsen (1879–1979). http://adb.anu.edu.au/biography/madsen-sir-john-percival-vaissing-vissing-7456

[13] Jack Piddington (1910–1997). https://csiropedia.csiro.au/piddington-jack-hobart/

[14] McCready, L.L., Pawsey, J.L., Payne-Scott, R. (1947). Solar radiation at radio frequencies and its relation to sunspots, *Proc. Royal Soc. A*, 190: 357–375.

sun was going up by orders of magnitude in power output at metre wavelengths. The question is: was the sun as a whole brightening or was this coming from the sunspots? Well, they solved that question by observation that radiation was not coming from the sunspots but it was coming from the vicinity and there's a clear correlation. The sunspots are really quite tiny, only minutes of arc across, whereas the source of the radiation was bigger and coming from a high-temperature region in the immediate vicinity and above the spots. They were able to track these sources day by day and show that they were rotating in exactly the same way that the sunspots were, so they really clinched that question.

Bhathal: In 1954 you were invited to give lectures on radioastronomy at the University of California. How did this come about and was this the beginning of your move to the United States?

Bracewell: Pawsey had met Otto Struve[15] at some meeting and, on returning from the meeting to Sydney, he had a letter from Struve asking if he would like to go and give a year's lectures at Berkeley. Pawsey couldn't possibly do that because he was directing a sizeable group, so he asked me if I would be interested in doing that. It was particularly appropriate because I'd just finished writing the book and I could easily speak on the whole of radioastronomy. I consulted my wife, Helen, and we had at that time a daughter nine months old and experts told us that nine months was a very good age for travelling with children because they weren't yet independently mobile so you could travel with them in planes and so on. I said, "It's only for a year," and she said, "Well, okay." I dragged her away from her family just for a year.

At that time I had no intention of moving to the United States. At the end of the nine months lecturing, which I found very congenial and several of the people in that class went on to make names for themselves in astronomy, I took the summer off and made visits to other astronomical places, in particular Ann Arbor, where I had friends connected with solar astronomy, the Department of Terrestrial Magnetism in Washington, where I had other friends and also met Merle Tuve[16], the director there, and also visited the U.S. Air Force Office of Scientific Research, who ultimately became my sponsors for many years.

While I was there, Struve said the University of California at Berkeley should get into radioastronomy. What was my advice? What should they do? Well, the background I came from meant thinking of something new, the whole record of radioastronomy with people thinking of a new instrument or a new target and following that up. It was not continuing earlier work. I thought about that and it seemed to me that the Christiansen array of 32 dishes and the Mills-cross idea of having linear arrays perpendicular could be combined. I wrote that down and gave that report to Struve and at the same time I sent a copy of it to Pawsey suggesting that it was something I might like to do when I got back to Sydney.

If you look at that correspondence now you can see I was far from thinking of remaining in the United States.

Pawsey's reply was that Christiansen was going to do it, so that left me without any-

[15] Otto Struve FRS (1897–1963). https://en.wikipedia.org/wiki/Otto_Struve

[16] Merle Tuve (1901–1982). https://en.wikipedia.org/wiki/Merle_Tuve

thing to do but I returned to Sydney early in 1955 and spent some time there. I discussed this with Christiansen and Pawsey, what to do next, and it was quite noticeable to me that I was not asked to join Christiansen's operation.

What in fact happened was that after three months I went back to Stanford where they offered me a teaching job and there was a reasonable prospect that I might get funding to do what I was going to do. I got funded, Chris had to move to the University of Sydney but he got funding, and we both built essentially the same array, mine working at 10 centimetres and his at 21. They were complementary in the sense that the things we did were observed at two wavelengths, and so you got spectral information. That's how I wound up in the United States.

Bhathal: Was it a very acrimonious time?

Bracewell: Christiansen was somewhat combative. He was a person who was very sensitive to injuries done to the working class. We had examples of that. For example, the rain physics people used to fly planes through dangerous clouds. The more lightning they had in them, the better. They were supposed to be measuring all this. They lost a plane at sea, so, all of a sudden, one of our colleagues doesn't come back, but they don't find any wreckage, see, so nobody knows for sure whether he's dead. You would think it would be only reasonable to wait for a week or two until some wreckage washed up somewhere. But his salary was cut off the same week that he crashed and his widow is left in a serious state, which you would have thought the management would have postponed an action like that until they knew for sure, until a court declares you're dead. If you disappear, you're not legally dead.

Christiansen was the person who took the initiative over that sort of thing. That sort of thing caught his attention, and he battled very hard over that to get some justice for the widow. That was typical of Chris. I forget the exact details of the argument he had with Bowen. It was irrelevant because Bowen needed to get rid of him because he couldn't fund him. Chris didn't want to leave, but then he told me that once he'd had this shouting match with Bowen he knew he had to leave; he didn't need to be told to go. He accepted the job as head of the EE department. He came and visited me. I helped him to fund a trip to the United States. He came and stayed for several months in our group and I just paid him salary out of my grant. I was relatively affluent then. He was a visiting expert and I just put him on the payroll.

I also helped Bernard Mills[17] who was in the same position of having to raise money. Mills got money from the National Science Foundation and I was able to support his application for cash there at the very same time that Bowen was trying to get money for the 210-foot dish, and it's not to his advantage to have the National Science Foundation giving money to Mills. I think you will find in some of the other historical accounts more detail of how Bowen tried to squeeze Mills out from funding but didn't succeed.

That's another example of Bowen's slippery personality. I always got on with him extremely well. When he was appointed to the Australian embassy as scientific liaison officer, he used to come to Stanford quite regularly, interview some students, take care of any Australian students, of whom there were only two or three. We always interacted in a perfectly friendly way, but he did have

[17] Bernard Mills FRS (1920–2011). http://www.eoas.info/biogs/P000648b.htm

these other aspects that one can criticise. It didn't apply to me. I was never involved in any acrimony.

Bhathal: In retrospect, do you think the Parkes dish was a good idea, then?

Bracewell: I think it's quite clearly proved itself. It wasn't Pawsey's notion of what to do next. He would rather think of ingenious new departures, and the idea of building a big battleship because a destroyer had worked wasn't his… and the management program was completely outside his way of dealing with things. He dealt individually and in detail with a sizeable group and could do the thinking and leading very well, but the idea of getting manufacturers and surveyors and going through land deals and so on, that wasn't his idea of doing science. But it was well within Bowen's range because he had seen the way radar had begun at Bawdsey Manor[18]. They had taken on a very big job with a lot of people and you certainly wouldn't micro-manage them. They all did their own thing but nevertheless there are big projects like that.

Bhathal: You have made several significant contributions to science and engineering. We want to look at some of these areas of research. Perhaps we should begin with your work on imaging theory in radioastronomy. Could you tell us about this work and the problems you were trying to solve?

Bracewell: That became my main preoccupation when I got into the radioastronomy. We had these well-known people, Mills, Christiansen, Wild[19], Bolton[20] and their fellow workers, very busy generating mathematical problems on the side. One of the problems that came up was if you were mapping the sky with a relatively large beam. I think J. S. Hey's[21] first survey of the sky had been done with a 17-degree beam. Well, that's not what we call high resolution. It was quite clear to Hey that the map that he was generating was not exactly the same as the true distribution. There's the measured distribution and there was what used to be called the true distribution. What one had in mind was that if you knew what was there and then scanned it with a 17-degree beam, it would be blurred and the blurred distribution would be what you measured.

The other way of looking at that is to say we don't know what is really there. All we know is what we observe, and we have to work back from that. Kevin Westfold[22], who was a mathematician and went back to being a university mathematician later on, produced a solution to the inversion problem. Given the observations, what was the true distribution? That solution came out in the form of a series and it occurred to me that when you were summing a series, when you were presented with a series, maybe it doesn't have a sum and, if it does have a sum, it's not guaranteed that it will be the true distribution. I gave that a lot of attention and I managed to solve the problem and I found that the series doesn't always converge but I found the condition for convergence which is often met and, if it does converge, then it never, if ever, converges to the true distribution.

[18] RAF Bawdsey.
https://en.wikipedia.org/wiki/Bawdsey_Manor

[19] Paul Wild FRS (1923–2008).
https://csiropedia.csiro.au/wild-john-paul/

[20] John Bolton FRS (1922–1993).
https://csiropedia.csiro.au/bolton-john-gatenby/

[21] J. S. Hey FRS (1909–2000).
https://en.wikipedia.org/wiki/James_Stanley_Hey

[22] Kevin Westfold (1921–2001)
https://csiropedia.csiro.au/westfold-kevin-charles/

This is a big jump. It's no good trying to get a solution if someone can show that actually you can't get it. That all worked out very well and we were able to show that this mapping problem was analogous to an electric circuit problem of filters, which at first seems to have no connection at all, especially as the mapping is two-dimensional and the electric signals are one-dimensional, but it proved to have a close analogy and we could use some signal theory which existed and then put in the presence of noise which had been previously left out. If you put in the noise, if you're not careful you're going to amplify it. That was a very sophisticated problem which I was very happy to… I got a lot of papers out of that because there are so many variants on it.

It has now been applied in many different subjects, and things like true distribution have disappeared. There was the spectral sensitivity function which turned out to be the two- dimensional analogue of the transfer function of a filter. In optics where similar problems arise, the word "transfer function" has now displaced whatever was the local usage in several subjects. "Transfer function" has become standard terminology binding all these subjects together and "impulse response" is another such term, even used in optics. Impulse of course is a mechanical thing but "impulse response" is understood in optics now. No-one thinks any more that these are all different problems but at that time we were just groping in the dark. So that was one thing in imaging.

Another one was Pawsey, McCready and Payne-Scott had reasoned out that there's a Fourier-transform relationship between interferometer observations and what is really there in the sky. With the interferometer, a typical record just looks like a sinusoidal wiggle, has a certain amplitude, has a certain period and it has a certain location on the chart. Pawsey was able to make a connection between such an interferogram and what the distribution of emission over a sunspot would be. He latched onto this Fourier-transform thing and showed that if you made a lot of interferograms with different spacings, a Fourier transformation would get you back to the thing you were really looking for.

That had a lot of loose ends about it. First of all, it wasn't two-dimensional and also one is applying a linear point of view to something that's really not linear. The sky situation is not linear, partly because the sky is curved and partly because of other reasons. I worked on that and managed to get the result in terms of a two-dimensional Fourier-transform relationship between the brightness distribution over the sky and the complex visibility distribution on the so-called (u, v)-plane. When I first did this it came out with u and v being used for the coordinates in spatial frequency and I'm happy to see that the (u, v)-plane is still current terminology today.

That was fun too because in optics there were similar things going on where we were measuring "visibility," a word which we got from Michelson[23], who had done the same at Mount Wilson. To him the visibility of interference fringes was something he saw in an eyepiece and he judged whether the visibility was 100 per cent, which meant that the minima went down to nothing, or whether it was 50 per cent and so on. We got that word from there and Michelson knew about the Fourier-transform relationship, which was arrived at quite independently by Pawsey.

[23] A.A. Michelson FFRS (1852–1931). https://en.wikipedia.org/wiki/Albert_A._Michelson

But in optics they had what they called the modulation transfer function. That was essentially the same as Michelson's visibility. It just measured the depth of modulation of the fringes with respect to the mean level if there were no fringes.

But to make this Fourier-transform relationship work, you needed also to know the spatial phase. This never applied to Michelson because when he saw interference fringes in his telescope, they were always drifting because of the atmosphere, and if the atmospheric turbulence increased, these things would drift faster and faster and then wipe themselves out. We needed phase. When I put the phase in, I got this more general Fourier-transform relationship and what was previously modulation-transfer function MTF in optics could be generalised to optical-transfer function, which was the same as the MTF multiplied by e^i times whatever the phase angle was. That work happened in optics at about the same time as it was happening in radioastronomy. Now it's all been unified and we've got a good general Fourier-transform relationship.

A criticism was made by Emil Wolf[24], a great optical wizard who was co-author of *Born and Wolf*[25], a great textbook still in existence, and growing each edition. He said, "No, you have something wrong there". It had previously been worked out by Zernike[26], and he had had to make an approximation in order to get the same result I got. I didn't have to make an approximation. Therefore, Wolf said, I must have made a mistake, but he couldn't say where. It's not that way at all. In optics the field of view was always so small that it was customary in discussing aberrations and whatnot to make some simple approximations which were absolutely correct for optical levels. We couldn't do that in radioastronomy. You had to work not in terms of angles but in terms of direction cosines. When you did that, it all came out exactly right.

The Fourier transform was done by hand at first. These days we have the fast Fourier transform. The group at Cambridge included H.M. Stanier. Stanier had made observations of the sun as a whole with a pair of antennas and he would come back the next day and change the spacing and he had several interferograms in effect. Then he superposed those and he avoided the problem of the phase of interference by arguing that since the sun is symmetrical, you don't have to measure the phase; you know it's zero. You know that all these cosine waves that he is observing must all have a maximum in the middle. You could say that was the same thing, it's part of the history.

Much the same thing was done by Christiansen in two dimensions where he had scans with his east-west array and he argued the same. He had someone in the room next to ours adding these cosine waves up by hand. I don't know how Stanier did it. I don't think he had more than half a dozen components, but Chris would have had 16 and they're being added up by hand. I watched this very carefully being laboriously done. It was very well known to everybody. I mean, when you look at the integral statement of the Fourier transformation, you can see. Here's the thing you're looking at and we're multiplying it by

[24] Emil Wolf (1922–2018). https://en.wikipedia.org/wiki/Emil_Wolf

[25] Max Born & Emil Wolf, *Principles of Optics: Electromagnetic Theory of Propagation, Interference and Diffraction of Light*, 7th Ed., C.U.P., 1999. Nobel Laureate Max Born FRS (1882–1970) was Olivia Newton John's grandfather.

[26] Frits Zernike FFRS (1888–1966). https://en.wikipedia.org/wiki/Frits_Zernike

a cosine function: you can do it one way and then you can see what nature is doing to you and you know you've got to do the opposite. The beautiful thing is that the opposite of the Fourier transform is the same as itself. You could understand this very clearly. Pawsey understood this. It was in the air.

Bhathal: From radio imaging you went on to do work in medical imaging. Can you tell us about this?

Bracewell: Yes. Well, see, about 1956, I think, I wrote the two-dimensional paper on reconstruction from fan beam scans. The idea was that you had a fan beam which would scan over the sun, as Christiansen was doing, and then you would get another scan at a later time at a different angle and then you would get scans at more angles. The question was: if you had all these scans, would you be able to get back to the original distribution?

That's a very tricky question. Christiansen was working with scans at a limited range of angles. You would get a scan at midday and then in the later afternoon the sun has turned a little bit in the sky, so that the scan you get is at an angle with what it was previously. For the medical imaging, we knew when the work by Hounsfield[27] was done at EMI, the same place that Pawsey came from, he was taking 180 scans at one-degree intervals. We then realised that if you're going to get a decent image, you have to have all the scans, but in the beginning you could only get a few.

But the theory of that was pretty tough and, since I had been working on Fourier things, this was one of the subjects Ratcliffe was well up on because the Cambridge crystallographers had worked this all out and he understood this Fourier stuff very well. I started looking at that problem and I published in 1956[28] a solution showing how you would get from the scans back to the original distribution. What's more, although the theory is done entirely in terms of Fourier transforms, the final algorithm doesn't use the transforms at all. It enables you to use the actual data and combine them together and you don't use transforms at all. This was really marvellous. You couldn't understand it without thinking in Fourier space but in the end you didn't use it.

In 1961, by which time I was at Stanford, we had been obtaining scans over the moon and thought this might be an opportunity to give it a try. We used that algorithm and it worked out quite well and then the next thing I know is that particular paper of 1961, which had been cited in the science citation index maybe once or twice and then disappeared from view, all of a sudden it becomes my most cited paper and it's been cited in journals of neurophysiology and Lord knows what. I don't know exactly how it came to be known, but all of a sudden all these medical people are citing it as though it was mother's milk, you see. You didn't really know it unless you gave this citation. I'm sure the great bulk of them couldn't understand the paper; it was pretty mathematical.

Somehow it leaked out and it might have been that all this time at EMI they knew about this paper in the *Astrophysical Journal*[29]. They were very close to Cambridge and they were obviously employing people who'd worked on radioastronomy at Cambridge. Some of

[27] Sir Godfrey Hounsfield FRS (1919–2004). https://en.wikipedia.org/wiki/Godfrey_Hounsfield

[28] Bracewell, R.N. (1956). Strip integration in radio astronomy. *Australian J. of Physics*, 9: 198–217.

[29] Bracewell, R. N. & Riddle, A. C. (1967). Inversion of fan-beam scans in radio astronomy. *Astrophysical Journal*, 150: 427–434.

these radioastronomers, who never emerged as radioastronomers but had been trained there, might have seen my paper. They'd had a few years in which to let it leak out. It's possible that Hounsfield had misled the commercial opposition by pointing out that in order to solve this inversion problem, you had to invert a matrix with 180 by 180 terms. Inverting matrices is pretty tough even now but 180 by 180, who would do that? But he had a way of doing it, he said, and that might have been complete eyewash. He might in fact have been doing it with the aid of my paper. I don't know. I've never met him and I'm sure he would tell me, but that's my private suspicion.

Bhathal: You constructed the microwave spectroheliograph. What was the motivation for this work and how useful has that been?

Bracewell: Well, that's essentially what we were talking about earlier. It was my first project at Stanford. It had various outputs. We made a weather map of the sun each day for 11 years. We also got Christmas Day. I mean, it was a seven-day-a-week operation. Those maps were distributed by tele-typewriter the same day that they were made and went all over the world and NASA made use of them for predicting whether or not astronauts were going to be fried by cosmic rays from the sun and avoid having any sort of activity outside, shut the windows, get inside and so on, and later gave an acknowledgment of this contribution to the lunar landing. Of course, we weren't trying to do that but it's rather nice to think… combining those observations with those of Christiansen at 20 centimetres compared with 10, we get an idea of the spectrum and of the distribution of temperature and density in the regions immediately in the chromosphere and slightly above.

Bhathal: The spectroheliograph study led you to make a study of Cygnus A. What was the significance of that?

Bracewell: Yes, we looked at Cygnus, but we looked at several point sources. That equipment was not designed for that purpose; it was for looking at the sun, which is a very strong source. We could only see a few sources, especially Cygnus and Centaurus and the moon. We did that to see what would happen. Of those, Centaurus was the one that proved to be far and away more interesting. Other people were able to study sources and map them with much bigger installations, so that was of no great significance.

Bhathal: This led to the discovery of magnetic fields, didn't it?

Bracewell: I'll tell you the story there. Centaurus A had been discovered by John Bolton and that would have been probably 1949, plus or minus a little bit, and he had named it. It's a very strong source and about five degrees across, a really big thing. Now, we had three minute of arc resolution, which is incredible resolution, so we did a scan across Centaurus and we found that the central source, which up till that point was thought to be a point source, in fact had two components very close together, about five minutes of arc apart, that we could separate that with our three-minute beam. We found they were not equal. We found that one of them was narrow and strong and the other one was broad and relatively weak, though they had a similar total flux density.

Meanwhile, Alec Little, working with the Mills cross in cooperation with Richard Twiss[30], who happened to be visiting, had

[30] Richard Twiss (1920–2005). https://en.wikipedia.org/wiki/Richard_Q._Twiss

had exactly the same idea. At a different wavelength they get a scan across Centaurus and they also see this central source and it's divided into two bits but, because of lower resolution, these two sources appear to them to be the same strength and the same width. Then Alec Little came to Stanford and worked in my group for a good year and while he was there he got a master of science degree at Stanford. I had known Alec since he was a boy, you see, at Radiophysics, and we always got on very well together. We looked at these things and saw immediately there's something interesting going on here; we need to know more about this central source.

The first thing you don't understand is, if you look at the photograph of Centaurus, it's a great big nebula, pretty nearly circular, with a dark irregular band of stuff running across its middle. Here we have found two point sources which are inside the visible optical thing. They're pretty close together, you see. We know the right ascensions of these two things and we can compare that exactly with the optical position and we know precisely where these two sources are but only in one coordinate, left to right. Top to bottom we don't know whether the one on the left is up in the north-east or whether it's down in the south-east. We've got two sources but we don't know in which direction the line joining them runs.

I then went to Sydney for a time. I was visiting the physics department and I went and saw Bowen and told him about this and he said, "Well, I'm going up to Parkes in a few days.

Why don't you come?". He was flying up in a small plane with a pilot and I found this very interesting. You go over the Blue Mountains and you head down towards Parkes and when you get there, there are sheep all over the runway. You do a U-turn and scatter the sheep and then you land in the hole between them, you see. I thought that was pretty cute.

I then arranged with Bowen to go back to Parkes and observe Centaurus with a new receiver that was installed operating at 10 centimetres. Brian Cooper[31], my old colleague, came up with me. I had been working on advisory committees for astronomical observatories in the United States, so I knew very well how, when the people at the U.S. National Radio Astronomy Observatory built new equipment and then outside visitors, who had every right to visit a national facility, came in and used the new equipment, it used to cause a little bit of concern because the people who did the real work that made the observations possible would sort of get pushed aside. It was very well understood that visitors had somebody with whom they were associated who would tell them what to do next and so on and would share in any publications. In my case that would be Brian Cooper.

We went up there and he was doing his own thing and I was up in the middle of the night making these studies. The first thing I found was that the beam width is now five minutes of arc wide and I'm trying to resolve two things which are five minutes of arc apart. It soon occurred to me that if you scan through that, which the big telescope was quite capable of doing in a simple raster scan, you would never see two blobs. The position of the blob would change with each scan but you would never resolve the two. I found out that it would be possible for me to scan diagonally by turning on one of the drive

[31] Brian Cooper (1917–1999). https://csiropedia.csiro.au/cooper-brian/

motors so that the antenna would be driving east to west and then at a certain moment I could turn on the declination motor also and with both motors going, the dish would run down in a diagonal direction.

Then we would turn off that second motor and the antenna would continue out to the west, then we would reverse that motor, have it come back in, switch on the second motor, go up another diagonal. In this way, two things which in fact were seven minutes of arc apart measured diagonally could be resolved. I get a long series of two or three nights of scans showing these two things and I can see they have different diameters and different intensities. But in addition to that, a means of rotating the feed horn had been installed and not as of then used on any serious problem. It was purely a technical thing out of the workshop at that point. They'd made it, they knew it would work but it hadn't been used by any observer.

Here I find I'm in the marvellous position of being the first person to turn the horn feed antenna. We repeat these observations. When I say "we," I'm up there in the dark in the tower and there's an engineer who turns motors on and off and makes sure nothing hits the ground and so on, and there's another technician who is keeping the receiver working.

That's Tom Cousins, whom I had known from years before. He had built the receiver and his duty was to make sure it worked when someone else was using it. Cooper is also there but he's sound asleep. I turn the feed horn and I discover that one of the two components is strongly polarised, much stronger than anyone had ever seen, 15 per cent. The other one was barely polarised, not detectable. This is pretty exciting.

I wrote that up in the visitors' book before I left and I'm happy to see no-one ever tore that out. In fact, two different people have sent me photographs of it from time to time and it's still there. It says exactly what I've told you: who was there, who did what and what the results were and how exciting it was. After I left, Easter weekend was approaching and Marcus Price, a young American, was there on his own and he knew exactly what we had done. I mean, everyone sat round at teatime and talked about it. Well, as soon as everybody went away for Easter, he took the feed horn out, replaced it with a 20-centimetre feed horn, connected it to the existing 20-centimetre receiver and repeated these observations. Now, at 20 centimetres, the beam width is about 10 minutes of arc, so he was unable to resolve the central component. All he saw was the unresolved source. Nevertheless, he scans through it, turns the feed horn and scans through it again, and finds there's a certain percentage of polarisation, quite strong, just as he expected, but the direction of polarisation is not what I got.

In a talk that he gave and is written up in the proceedings of a meeting that took place at Charlottesville not too long ago — ten years ago perhaps — he said, "I realised poor old Ron got the direction wrong". He's a humorous sort of fellow. He goes back down to Sydney really excited and the first thing he encounters is he's reprimanded for having used the telescope without permission. Here he has made this great discovery and they're telling him, "You're not allowed to do things like that. You're an underling and there's no-one there in authority. How dare you do such things," you see.

Brian Cooper was pulled in and Brian thought that this disagreement in the two position angles of polarisation might be due

to the Faraday effect caused by magnetic fields in the region either in Centaurus or in the intervening space. In order to establish that beyond doubt, he made observations at at least three different frequencies and showed that the rotation went in proportion to the wavelengths squared, which is what it was supposed to do, and he found that and he wrote that paper up. That's *Cooper and Price*[32]. Meanwhile, I had written mine up and handed it in for typing to the Radiophysics lab. They had typed it up, just handed it to Bowen. Bowen rewrote the first page and I simply adhered to that. I might have changed the grammar a bit. His motivations in dealing with the outside world from the point of view of a director are not quite the same as mine and I can understand why he is making these minor changes. He refers to this, that and the other in a way that makes Radiophysics look a little better relative to work going on at other labs. I've still got this handwritten stuff and the final version incorporates his modifications.

It went off to *Nature* and in due course I see it appears in *Nature*[33] but to my surprise, first thing is I've acquired a third author. I've been diluted by an extra 50 per cent, you see. Tom Cousins appears on it, never previously mentioned. The other thing is that *Cooper and Price*, the work that was done second, comes out in print first — some skulduggery went on here. Brian assures me that there was no skulduggery but it was very odd. For years, this didn't bother me at all, not a moment, because I knew that it would be apparent from the dates on which the observations were made and the date of receipt of these manuscripts at *Nature* would make it clear to anybody what the sequence was. It was only quite recently, just three or four years ago, when looking at this material again I realised the dates of the observations and the dates of reception are not there. How that got done I really don't know. I mean, that is really sophisticated. The fine detail of what happened there is not known.

The next thing, what really got me interested, was Alec Little then sent me a clipping from the *Sydney Morning Herald* with a picture of Bowen, saying, "Great discovery made by local people, marvelous," and Bowen saying, "Yeah, this is the biggest discovery we've ever made in Australia, polarisation in Centaurus," and it raves on and on. Alec Little's handwritten note attached to it says, "They really think your work was really marvellous. Pity they didn't mention your name." That really got my attention. That is why I wrote up my internal report. Since two or three other reports like this appeared in print, I thought, "I'll write up my version." I might even have sent you one. I know I sent it to various people. That's the story of Centaurus.[34]

Bhathal: In 1971 you constructed a second radio telescope.

Bracewell: That was much bigger, five 20-metre dishes extended over a bigger baseline and with a very narrow beam width. We made observations of a variety of galactic sources and published those. For technical details, I think I'll just refer you to the lists of publications. From that we got various

[32] Cooper, B.F.C. & Price, R.M. (1962). Faraday rotation effects associated with the radio source Centaurus A, *Nature*, 195: 1084–1085.

[33] Bracewell, R.N., Cooper, B.F.C., & Cousins, T.E. (1962). Polarization in the central component of Centaurus A. *Nature*, 195: 1289–1290.

[34] Bracewell, R.N. (2002). The discovery of strong extragalactic polarization using the Parkes Radio Telescope. *J. of Astronomical History and Heritage*, 5: 107–114.

other returns. For instance, going back to the cross antenna, we had the problem of making wave guide runs equal to about a millimetre in 100 metres. That's one in ten to the five. That is what we consider to be geodetic survey accuracy. But you can't really do that very easily and we worked out electrical ways of making that sort of length measurement.

Then we trained a lot of people who then went into radioastronomy in other places and various items of technique, not to mention these image-forming problems, were solved. Much the same applies to the five-element array of 20-metre dishes. All sorts of things were ironed out that were dubious when we began. It was a hard thing. It was one of the first aperture synthesis arrays working by earth rotation. Christiansen was the very first person to demonstrate earth rotation synthesis, then Martin Ryle came along and invented the term "aperture synthesis" — didn't mention Christiansen — but there were still loose ends when we'd come along and we managed to get that working.

About 1970 there had been an explosion of university radio telescopes. There was a move afoot to bolster the national radioastronomy centres. There were two at that time, one in Charlottesville and one in Puerto Rico at Arecibo, and people were arguing in Washington that we couldn't afford to have new expansions of expensive equipment in many universities, much the same as has happened in particle physics, and that the national centres should be boosted. I got a telephone call in 1970 from my sponsor, the National Science Foundation, saying, "I don't know how to tell you this, Ron, but the National Science Foundation is not going to sponsor your radioastronomy research any more."

There are some funny aspects to this. I wasn't the only one who got a call that day. Harvard was shut down, Michigan, Ohio State and one of the Florida universities, and I think Berkeley too. In the case of John Kraus[35], who was at Ohio State, the conversation went like this: "I don't know how to tell you this, John, but we're not going to support you any more," and there's no answer. "John, are you there?" There's absolutely no answer. The fellow got quite alarmed. He telephoned someone that lived in the same building and said, "Go and see if John Kraus just dropped dead." It took them quite a few years to shut Kraus down and the same is true of me. I fought back. I knew it was illegal. The National Science Foundation is there for the purpose of reading your proposal and rejecting it if they see fit, not to reject it before you've submitted it.

I fought back and I hung on for another three or four years, but this is the sort of thing that would happen. We needed more sensitivity, we needed some special sort of receiver. These words were on the tip of my tongue for years. We needed parametric amplifiers, we needed five, one for each dish, in order to boost our sensitivity and we'd be able to see a lot more objects. I wrote a proposal and this is the sort of thing that would happen. You've got to understand this peer review system means that your competitors are your peers. They're the ones who know what you're doing and any money they give you is less for them, you see. Here's a typical thing. NSF told me that a reviewer said, "It's a very poor proposal. Even the wavelength of observation is not mentioned." I replied, "Please note that it's mentioned in the abstract, on page 1, page 3, page 5,

[35] John Kraus (1910–2004). https://en.wikipedia.org/wiki/John_D._Kraus

page 7 and page 9. Please take a note of this reviewer's name." By this time NSF is explaining why I didn't get funded, you see, it is too late — a done deed.

Another reviewer said, "The trouble with this radio telescope is it's not sensitive enough, why should we give you things to make it more sensitive?". You see, the logic tends to irritate you. Anyhow, the writing was on the wall and year by year I would lose another colleague and I began to notice that each time one of my colleagues left and went somewhere else, it wasn't as traumatic as it seemed. They all got good jobs mostly in radioastronomy elsewhere, but not all — some in industry. Each time one went away, the quality of life went up because we wouldn't be sitting around figuring out what would go in on our next proposal. Finally when my secretary went, the quality of life really hit the ceiling. I didn't have to rush in to the office and create work. Radioastronomy built up to a big maximum and then slowly faded away and went to the national centres.

I have another strange thing to tell you. In these days when I was trying to raise support from my friends to work on NSF, I went to Charlottesville and was talking to the director there, Mort Roberts[36], who had been a student of mine at Berkeley, and I'm telling him what I'm telling you, you see, and he's very sympathetic. Then, as I get up to leave, he says, "By the way, do you have any students that we could take?" He'd completely missed the point. I'm not going to be able to produce any more students. I've produced my last student, and it really hasn't struck him that closing down universities in favour of national centres, of which he is it, is going to cut off their source of supply and they have to become universities themselves. It was a turning point in life but we survived.

Bhathal: You received a patent for your work on the Hartley transform. Can you tell us about this and its usefulness in scientific work and how different is this from other methods?

Bracewell: The Hartley[37] transform is like the Fourier transform. It is fully equivalent. If Smith gives us a function, you take the Fourier transform, I take the Hartley transform, we both have identical information. Anything you can do I can do and vice versa. The difference comes about in the calculations that you will have to do. You will be doing complex arithmetic and I will be doing real arithmetic. When you come to put your stuff into the computer, you'll find that your computer doesn't multiply complex numbers, it multiplies real numbers. That will be very irritating. In fact, when you've finished doing all your complex multiplication, you'll find that you've done twice as much as you had to do. The reason for that is that the Fourier transform of a map has symmetry. The value at any point in the (u, v)-plane is the same as the complex conjugate of the value at the opposite point, or the point that's 180 degrees away.

You've done twice as much work as you have to do and that's because complex numbers are a creation of the brain. They are not in nature and nor did Fourier think they were either. He multiplied by sines and cosines, not by this e^i theta stuff that is very convenient for human beings who understand complex arithmetic. But it's not essential and the computers don't like it. The

[36] Mort Roberts (1945–2010). https://www.nrao.edu/archives/Roberts/roberts.shtml

[37] Ralph Hartley (1888–1970). https://en.wikipedia.org/wiki/Ralph_Hartley

effect of the Hartley transform was to permit you to do the FFT in half the time. When I sent in my paper on that, the reviewers really couldn't understand how that could be. They knew they'd been perfecting the FFT for some years, and only discovered a little later that Gauss had done it back in the early 1800s, and they could not understand how you could cut the time. They'd been whittling it down 5 per cent, 10 per cent, year by year and they'd really got to the bottom. They were doing prime factor algorithms, they figured out all sorts of quaint things, but they couldn't find out where the paper was wrong.

I got three lengthy reviews criticising minor things here and there and I dealt with all of those. Then I got three more lengthy reviews, in the same vein. I knew my analysis was correct. They couldn't find out where the paper was wrong. They knew it was wrong. Then finally I got a letter accepting it and saying it would come out in April. Two years go by. This is my record on delays in publication. Before the month of April comes, I get three more negative reviews from the IEEE. I don't know how this … I thought I'd better not ask, I'll wait until April comes. When April comes, there it is in print[38]. Where these three extra reviews come from I don't know, but a lot of people were inconvenienced by it. Then when the university Office of Technology Licensing said, "Why don't we take a patent out on it?" that went through and also made a milestone. It was the first patent with an embedded copyright. What this means I don't want to explain to you now, but it had never been done before.

A howl went up from people. "How can you possibly be taking financial benefit from something given by God to everybody? It's not your property. You didn't invent it," and things like that. It was really funny for a while. It didn't do me any harm. Although the cash take was to be divided three ways between the dean of engineering, the chair of electrical engineering and me, they never told me that they don't do this three-way division until the Office of Technology Licensing had paid off their out-of-pocket expenses, which were the salary of the person principally responsible for at least six months, maybe a year, and the fees of the attorney. Nothing came at all for several years and then I got maybe a thousand dollars out of it.

The fact that it works twice as fast has become irrelevant because each year the speed of computing goes up, practically doubles, and I'm pretty sure now that hardly anybody is using the fast Fourier transform. The slow Fourier transform is just as good. It takes a tenth of a microsecond and it used to take 10 microseconds. What's the difference, you see?

Who is using the Hartley transform now I don't know. Hundreds of papers appeared, literally hundreds, and we know for all the people that write papers there are other people who are using it, but where and who they are I have no idea. All I can say is that I've caused Mr Hartley's name to be transmitted in perpetuity. Oxford University Press commissioned a monograph *The Hartley Transform*[39].

Bhathal: With the advance of the space age, you were involved in some experiments on the Sputnik. Can you tell us about this work?

[38] Bracewell, R.N. (1984). The fast Hartley transform. *Proc. of the Institute of Electronic and Electrical Engineers*, 72: 1010–1018.

[39] Bracewell, R.N. (1986). *The Hartley Transform*. New York, O.U.P.

Bracewell: Well, we were having dinner one night with Villard, Peterson and Eshelman, colleagues in electrical engineering, and the telephone rang and said, "The Russians have just sent up a sputnik and it's arriving over California in half an hour," or something. "What do you know about it?" I said, "Well, I'll tell you when we've gone and had a look." We're all having dinner and the men all jump up and zip off to a field shack and get antennas ready. They told us the frequency, about two megahertz, and we hear it go over. No question. It's going "beep, beep, beep." Then the question was: was this a Russian fraud? Was it really up there or was it only apparently up there? Well, after it's been around two or three times, it's quite clear that it's up there all right.

I got interested in that and with Owen Garriott[40], who later became an astronaut in his own right and spent a lot of time up in space, we took recordings of the satellite transmission and found it was modulated in a funny way and we could attribute part of this to the ionosphere and Faraday rotation and partly due to the rotation of the satellite itself. It's rotating. It's doing a very funny sort of rotation. We got the theory of that all worked out. I don't know how you heard about this, but Explorer One was launched after the east coast scientists at the Naval Research Laboratory had failed two or three times to launch their grapefruit-sized satellite. There was no shame in that because they were competing with Germans who'd been doing it for twenty years.

They got Werner von Braun and said, "Listen, what are you going to do about this?". He had very cunningly put a Redstone in a garage and was waiting for this day and when the day came, he wheeled it out, lit the wick and there's a satellite in orbit. That's experience, you see. At the Jet Propulsion Laboratory they were trying to launch much smaller things and they built the Explorer One, which was about two metres long and about 15 centimetres diameter, and on the launching pad they set it into rotation. They had a rotating turntable, so it is actually spinning before launch. Their idea was that it would behave like a rifle bullet. It would keep pointing in the same direction. That was important because it had two dipoles at right angles and the thing is spinning but it's circularly polarised and so the spin doesn't matter.

To their surprise, when it comes back — after one trip around the earth it comes back over the Jet Propulsion Lab — it's like a ski thrown across the ice. It's slithering across. The next time it comes back it's turning head over heel and doing dreadful things. Since we'd been thinking about rotation, it didn't take long to figure out what's going on there. If you have an object that's long and narrow like that and spinning, as it loses energy, since it can't lose angular momentum, the energy has to be taken from some mode of vibration or rotation that does not contribute to the angular momentum. If you have something that's pencil shaped spinning around its long axis, it will wind up rotating about an axis perpendicular to the axis of the pencil. But if you have a disc-shaped satellite, it will rock as it spins but the rock will die out, leaving the pure spin, so it will stabilise itself. That turned out to be of some excitement for the time being. It was rather enjoyable to do.

Bhathal: In the late 1970s you invented the spinning nulling two-element interferometer, which I understand is to be used by NASA in its program to search for earth-

[40] Owen Gariott (1930–). https://en.wikipedia.org/wiki/Owen_K._Garriott

like planets. How does this instrument work and what was the motivation that made you invent this?

Bracewell: Barney Oliver[41] was interested in the search for extraterrestrial intelligence and got elected head of the IEEE, the world's largest professional organisation. He spent his year as president travelling around the United States giving lectures on how to design a huge antenna array that would pick up messages from extraterrestrials. Then he got support from NASA to hold a summer school which attracted science teachers from all over the United States to work on some project. He had them designing this huge array, hundreds or thousands of dishes as big as the biggest dishes then available.

I was asked to spend some time working with this group and I was overwhelmed by the size of the project and I thought there might be some other way of doing it. I proposed that an interferometer ... well, what I'm trying to do is to detect planets, not the extraterrestrial intelligence people themselves, but to detect those stars which might have planets. The difficulty here, the technical difficulty, is to blot out the light from the sun and see the very much fainter light from the planet. Barney Oliver himself had found out that you could design a telescope mirror not much more than a metre in diameter which would be so free of side lobes that you could see planetary light even in the presence of the strong light from the star.

But I thought if we could use an interferometer, then we could have the components in anti-phase suppress the light from the star and then arrange for the maximum sensitivity to be in the general location where you imagine planets might be. At that time the Space Shuttle was under development and we knew exactly what the size of the instrument bay would be, so I designed this so it would fit in the instrument bay of the Space Shuttle. That was really amusing because I already knew from experience of friends that if you wrote a proposal to NASA, five years would go by before it could be all engineered and designed and then another five before it could fly, and it never occurred to me that this was anything more than a talking point, a sort of joke even.

We worked out a lot of the details of that. If you have an interferometer with null reception along a line, you can put that line straight through the star. Mind you, that's going to take very precise pointing, but that had already been reached, or was nearly good enough. It certainly can be done. If you then rotated the interferometer and let it spin, the planet would go round and round in the pattern of the interferometer and cross through the null line twice per revolution. If there was a planet there, you would see modulation at a frequency which you yourself had injected into the instrument. That was the idea.

I think I got something like $10,000 or $15,000 from NASA to pay salary to a visiting scientist, R. MacPhie, who came to work with me at Stanford at that time. After we had written our report, NASA gave our report to Lockheed and gave them $100,000 to report on our report. Their report on our report was based entirely on what we told them face to face. This was a sociological comment. It turns out that the nulling interferometer has now become a reality. The multi-mirror telescope on Mount Graham was disassembled about a year ago and, when the last two remaining mirrors were in place, was used as an interferometer at infra-red

[41] Barney Oliver (1916–1995). https://en.wikipedia.org/wiki/Bernard_M._Oliver

wavelengths, just as planned, to produce a null on Betelgeuse and was successful in suppressing the starlight and, lo and behold, they were able to see the infra-red glow from a cloud of dust similar to that which creates the zodiacal light on earth.

It not only proved the principle of the instrument but also made a minor astronomical discovery, and that's from the ground. The idea is that that will ultimately be flown in space, not only in space but out near Jupiter. I mean, it's a really large undertaking. It's in the hands of Roger Angel[42] and Nick Woolf at Tucson and is one of the two serious contenders for NASA's next major investment.

Bhathal: What are the dimensions?

Bracewell: As I saw it, it was going to be about 10 metres long. Angel and Woolf, since they don't feel they're restricted to the bay of the space shuttle, have doubled the length and added two components, so it's now a four-element which has certain technical advantages, and they've also moved to a wide band of infra-red which will enable them to see water vapour, oxygen and CO_2. Not only will they pick up the radiation from any planet they happen to detect but they will also get an indication of the atmospheric components. If they see oxygen, for example, that would be a strong indication of life because oxygen in the earth's atmosphere is entirely due to biological activity. Before there was any life on earth there was no oxygen in the atmosphere.

Bhathal: You were also interested in the SETI program[43], the search for extraterrestrial intelligence programs. You were a little critical of the Drake equation. What is wrong with the Drake equation from your perspective?

Bracewell: The Drake equation[44] seems to get quoted by all writers in the SETI business but it can't possibly be right. First, I'll tell you what it does and then I'll give you a homely example. The idea is we want to know the number of intelligent societies in the universe, *N*. We say that *N* is equal to the rate of formation of stars multiplied by the probability that the star will have a planet at about the right distance from the star where water will be in liquid form, and we multiply that by the probability that life will start and then the probability that it will reach technological level, and you throw in a few more factors such as the longevity of a technological civilisation and then you get an answer.

Somehow, they always seem to arrange this so that it's a pretty good number, looks very encouraging. Here's my criticism. The fact of the matter is we don't know the values of any of these parameters. Suppose I were to say to you, "Let's estimate the number of cats in Sydney. *N* is the number of cats in Sydney. It's equal to the rate of formation of cats (the birth rate) multiplied by the longevity of a cat multiplied by the probability that the cat will be fed so it doesn't die at birth and multiplied by other probability factors connected with the funding of the RSPCA and things like that". If you're going to have to guess the rate of formation of cats, you might as well just guess the number of cats. What's the difference? I mean, how many kittens are born per day? You'd better tell me how many mothers there are before you start guessing that.

42 Roger Angel FRS (1941–). https://en.wikipedia.org/wiki/Roger_Angel

43 https://www.seti.org

44 Frank Drake's equation, 1961. https://en.wikipedia.org/wiki/Drake_equation

Furthermore, there's more than one way of getting cats, you see. There are cats that grow up in loving households and there are other cats that are fed by secretaries and grow up under the buildings and then there are feral cats, you see. It's not all produced by one procedure, there are several. My fundamental criticism of the Drake equation is it does not contain any plus signs, so it cannot be right.

Bhathal: But cats are living systems, whereas the Drake equation refers to physical things. Is there a problem there?

Bracewell: Well, it refers to life, you see. It's the number of technological civilisations. There's a very good chance that we're the only one in the whole universe. Of course, you might have a personal bias against that. If you're conducting observations intended to locate these people and they're not there at all, you wouldn't be very receptive to the notion that we're the only ones.

Bhathal: You wrote a book called *The Galactic Club*[45]. Can you tell us what you were trying to tell your readers?

Bracewell: I was pursuing the consequences of different rational lines of reasoning. You could say that since our sun has planets, other stars would have planets. Until recently that was really not known for sure, and in fact it's not known that other stars have planets like the terrestrial planets. All they're observing now are planets like Jupiter which we don't think are very good habitats for life. But let's assume that other stars do have planets like ours. Our planet developed life, so maybe they did too. In that case some of them would have developed life long ago and some will just be in the age of monkeys, some will be in the age of trees and fungi and others have no life at all.

Of these various other supposed living societies, some would be more advanced than we are, and in fact not just by a hundred years. A little more than a hundred years ago, we didn't have radio, so it would be pointless to try to communicate by radio in those days.

But there'll be societies somewhere else which are thousands of years ahead of us. When we see the fantastic burst in information sciences lately, you can only imagine that they will be … we cannot imagine the abilities that they will then have. The notion that we should take the initiative in making this contact doesn't seem very plausible. It seems that they would have very much more power and they will be taking the initiative. In fact, when we first make contact with an external civilisation, why should that be the first time it's happened? It would have been done before, perhaps many times. The first small group to get into contact would then begin to organise making contact with other groups.

That's the galactic club, you see. It's the supposed group of intelligent civilisations that are already in contact. Perhaps they are trying to contact us. They have had experience. When you say, "Well, where are they? There's no sign of them," you could say perhaps they checked on us a couple of hundred years ago and they found no signs of radio waves, which would be a very easy thing to discover because you would simply listen with a sensitive antenna and you would hear none, so you would know we had none. They might say, "Okay, cross them off the list and put them back on in another couple of hundred years," or another thousand years.

[45] Bracewell, R.N. (1974). *The Galactic Club: Intelligent Life in Outer Space.* Stanford, Stanford Alumni Association.

I mean, what's the hurry? We've had life on this earth for 3,000 million years, maybe 4,000 million, so what's the hurry? If they wait for another hundred years, we'll be better prospects for contact.

That's the idea of the Galactic Club. But now go back and suppose that our original suppositions are not right. We're assuming that we would be average. There'll be some ahead of us, some behind us. That's an assumption. Let's test it out on some particular case. Here we have life on earth. We have everything ranging from ants to elephants, so therefore we should be average. We're not. We're at the top of the tree. We are unique on this earth. This assumption of mediocrity, as it's known, fails in the one case we can test it. It's just as conceivable that in the whole universe we are the top of the tree and we are in fact the instrument by which the whole of the universe is going to be populated with intelligence. Our destiny might be unique and we are the beginnings of the galactic club if there ever is going to be one. I was trying to balance these two alternative views and the one that we're unique is not popular among the SETI world, although I must say they're all friendly. In fact, we're a small club, you might say.

Bhathal: You mentioned in your book how long it would take to populate a galaxy. How long will it take to populate a galaxy?

Bracewell: Well, not very long by cosmic standards. It's about 100,000 light years across the galaxy. If you travelled at the speed of light, it would take you 100,000 years. If you travelled at a tenth of the speed of light, that would be a million years. If you travelled at 0.1 per cent of the speed of light, that's 10 million years. Now, 10 million years is microscopic compared with the time that life has existed on earth. We'd be there. To think about that another way, the earth is populated with human beings. Only Antarctica had no humans and a few Pacific islands when the explorers began covering the earth a few centuries ago.

Well, how did all these people get there? You could imagine that life could originate in Africa. It could also originate in Siberia. The bears could have come out of the Arctic forest, adopted an upright stance and begun hunting mammoths and developed language and so on. In South America the three-toed sloths might have descended from the trees and adopted an upright stance, developed language, grew very long legs for chasing after the game animals. You'll notice that, although they had the opportunity to do this, what actually happened was that the superior form that originated in Africa, so we believe, migrated on foot to these far corners of the earth. The people that Charles Darwin found in Patagonia had walked 10,000 miles from Africa not as individuals, but their grandparents and so on had marched. If you say they wouldn't travel much more than a mile a year, why would they? They settled in a certain place until they've eaten everything that's eatable and then some of the boys and girls go over the mountain and start another colony somewhere. To cover 10,000 miles from Africa to Patagonia would take 10,000 years on this very rough calculation, and 10,000 years, you see, is negligible compared with the million years that it took for human beings to evolve from their antecedents. The notion that we might populate the galaxy has to be taken very seriously. That's why of course we're all interested in the SETI activities because, if they were to produce evidence of intelligent life, this would help us understand what this universe really is about.

Bhathal: What's your answer to the question, where are they? That's the question you posed.

Bracewell: Yes. Well, there could very well be some other communities like our own in the galaxy, so we say, "Why haven't they arrived here?". Well, why would they? We have no idea of their values or their form of life. We see that there have been human beings who like to explore, mostly driven by greed for gold, some for other purposes, but if you look at the motivations of the explorers, it had largely to do with commerce if they knew roughly where they were going or some other commercial motive. We have pure explorers. I dare say one can think of a few but you have to think pretty hard. You might say those Franciscan monks that came from Europe and explored South America were doing it out of a pure sense of exploration, but even that is not clear. They brought military men with them and they had religious beliefs, it wasn't just scientific curiosity.

There are plenty of reasons why there could be other people out there and they haven't arrived or maybe they're due here in another thousand years, as I mentioned earlier. I don't think that it's a very cogent negative comment at all.

Bhathal: Forty years have passed in the search for ETI in the radio spectrum but nothing has been found. Should we be looking at other parts of the electromagnetic spectrum, for example, the optical infra-red spectrum?

Bracewell: There's a lot to be said for optics and there are several other modalities that you can think about. For instance, the magnetic field lines that extend out of the solar system go somewhere. If you were to shake one of those field lines, there'd be a ripple come through our solar system. It wouldn't be visible and it wouldn't be audible in the sense that radio is, but you can send influences by a variety of strange ways and for which we might in due course have ways of detecting. I don't think personally that expanding into the optical will make much difference. After all, infra-red and radio are much the same, so maybe there's a factor of two you might gain.

No, I think that the way this is going to open up in the future is that we're going to find there is life pretty widely distributed but it's going to be in bacterial form, elementary, and that it's basically everywhere. This would explain why when the earth was extremely hot and totally unlivable and being beaten up by meteor impacts, then when the meteors mostly landed and been cleaned up by hitting the earth or the moon — most of them would hit the earth compared with the moon because the earth is more massive — as soon as this surface of the planet became habitable, there's life there. That suggests to me that the strange coincidences that are required for life to generate itself didn't happen on the earth. Something happened somewhere else and that creatures or some sort of particle capable of living on the earth fell onto the earth from outside.

Bhathal: You have at one time suggested that advanced civilisations might be using probes. Can you tell us more about this idea of yours?

Bracewell: Well, I think it's a very good idea. It was based on the assumption that there is a galactic club. Now, there's an engineer in charge of making contacts with other civilisations and he's got a budget: he has to spend his money in the most efficient way. It's true these people might be incredibly rich compared with us but he's being told, "Go

and contact more civilisations," and since they already have contacted a lot, it's not all that important to them, but he's got to do it cheaply. He thinks about the SETI by radio and he knows the sort of record that has: he has to consider sending out a probe. Now, he would send out, let's say, one probe a year. It wouldn't cost much. It would be about the size of a human head or, if their heads are smaller than ours, about the size of their head, and relatively cheap to launch. It would take hundreds or maybe thousands of years to arrive but it would be launched in the direction of a star like our sun which they know has a reasonable chance of having a planet, and very possibly they are able to see these planets.

The probe arrives and it spends a year or so travelling through the solar system and its duty is to attract our attention. If we have radio communication, which we do, that visiting probe would be able to receive the transmissions from our radio transmitters and television transmitters. It now knows that there are intelligent people here. We may not be intelligent but at least we're technological. Its next duty is to inform us that it is here visiting us. This may seem to raise problems of language but in fact is very simple. All it has to do is to pick up, say, the 6 o'clock news, amplify it and re-radiate it. People all over the country would hear not only the person reading the news but with a time delay of some minutes or even hours, depending on how far out in the solar system it is, they would hear an echo. This would be the first time they ever heard it and people all over the country would hear it at the same time and it would be obvious to technical people that it was an echo and of a very unusual kind. The location could very soon be established and it would seem to me then to be up to us to acknowledge receipt of this message.

There would be some people arguing that we shouldn't acknowledge receipt because, if we gave away our position, they might land and dig up our gold and so on. However, they already know we're here and you might not want to respond, but somebody would. The way to respond is simply to re-echo what they have already echoed. They see that you're on to it and you understand it and then they're going to exchange information. This seems to really involve a problem of language but in fact it's a question of television. They already know the parameters of our television transmissions — not the same in Europe as the United States but they can easily cope with that.

They will send us, as I imagine it, a picture of a constellation, say Orion, and we see this on our television screen as a recognisable constellation. Now, it might be back to front or upside-down but we'd still recognise it. I can just imagine a group of important people standing around looking at this and they're saying, "Yes, it came from Orion, but which star?". Maybe Betelgeuse would then begin to blink. That's where they've come from.

They've told us without any knowledge of English, you see, only with pictures. Then you could imagine that we have a marvellous movie in which they zoom in on Betelgeuse and then we see the planets and then we zoom in on the planets and then we zoom in on the people. What a wonderful thing that would be. I think it's very exciting. I'd be delighted to be present when that happens.

Bhathal: You have been conveying an appreciation of the role of science in society, the public. Could you tell us what you have

done and how successful have you been in this venture?

Bracewell: Well, I have given lots of lectures and written lots of things. In a way you could say it's disappointing, especially as you see that many scientists have put considerable effort into writing books, writing newspaper articles, yet when we look at the people in government and in law, we don't see that major problems of society which have scientific components in their solutions, we don't seem to notice that the law-makers are very well grounded in the relevant science. You could say that about global warming at the present time, for instance, but about many other things. We don't seem to have a population led by people who understand things that are clearly of vital importance to us.

Problems of health which would be helped if we made new discoveries sometimes don't receive the support they need. Money is spent instead on treating symptoms. You could say that about tobacco. It's killing a lot of people and we think of ways of alleviating the symptoms but what we do is not entirely logical. It's not just plain science, it's logic.

After thinking about this for a long time, I don't see how it's going to change. I think in the end we're going to get what we deserve. We're going to bumble into things and when there's blood on the road a millimetre deep everywhere, we will start working on speed limits for cars and things like that. But if you don't personally see people being run over, the fact that there are thousands of them doesn't get your attention, so we don't do the logical things. We wait until we feel personal pain and then we do something. In many cases that could be too late, especially on global matters where population concerns and food supply are going to create devastating things in other parts of the world before the advanced countries who are in a position to do something about it technically begin to even pay attention.

I'm a little bit gloomy about science education. The science sections in the newspapers have improved a lot but people are just not interested in reading that sort of material. It really looks as though we are waiting for an oligarchy of very wise people to run the place but, since that's been tried in past years and we know that doesn't work, I think we have to get back to what Winston Churchill said, that democracy is a lousy way of running a country but it's the best considering the alternatives.

Bhathal: I was interested to read that you gave the 1996 Bunyan lecture on the destiny of man[46]. What is the destiny of man?

Bracewell: Well, of course we don't know. There are two extremes. One is that we're going to survive and as consecutive difficulties arise, we'll deal with them and can continue on — in my opinion, controlling the population is the most serious one — or we'll fail to do it. I saw a rather sad quote from Jacob Bronowski[47] who said that our destiny might be controlled by failure of the human mind. He doesn't mean the failure of individual minds. He means failure of the mind of the community. That is really a worrying thing. I myself don't see a way in which we're going to cope with the fact that half the people are below average and that of the representatives that are elected, half of them or maybe more are below average.

[46] https://web.stanford.edu/dept/astro/bunyan/bunyan-1996-poster.pdf

[47] Jacob Bronowski (1908–1974). https://en.wikipedia.org/wiki/Jacob_Bronowski

Bhathal: You have written several books. I wonder whether you could give us an insight into some of these books. What was your motivation in writing them?

Bracewell: When I got my copy of *Pawsey and Bracewell*[48] — that would have been in 1954 — I got a very warm feeling. I had put in a lot of work on it and really worked hard but when I saw this and felt it in my hands, after having read many other books written by other people, to have one that you contributed to yourself, I had a good feeling. That continues to be the case. I haven't brought out very many books but it gives you a pleasure to do that. It's a sort of reward. It doesn't seem to matter too much how many people read your book. You will notice a lot of books are printed and clearly the authors got this warm feeling that I'm referring to but, even if it only sells 500 copies, I think they get the same pleasure as one that sells five million.

Bhathal: After a lifetime devoted to science, what do you consider the major achievements in your life?

Bracewell: Well, you have to be pretty modest in things like this, but you can discuss it. My contribution to medical imaging could perhaps be measured in dollars or in human life or something like that, yet it's out of all proportion to my estimate of the work I put into it. Something might be valued in accordance with something measurable, like man years of life saved. Who knows? But was it a major achievement? In any case, one only makes a contribution. If I hadn't done what I did, it wouldn't have delayed things more than a couple of years at the very most. It's not a great achievement.

On the other hand, things that I thought were pretty clever and difficult and I got done, I'm not sure whether you'd call it an achievement, you see — never heard of or never had any impact. It's an interesting question. You could certainly ask people what their major achievements were and very likely get a list in many cases, and then you could analyse that list and see how much of it they contributed themselves and what impact it had on society, how difficult was it in the sense that if they had not done it, it wouldn't have been done for years. Take Wegener's hypothesis of the drifting continents.[49] That was a fantastic achievement as measured by the fact that it took 30 or 40 years and no-one else did it in the meantime. That tells you it was hard. It's a good question but there's really …

Bhathal: Maybe we should rephrase it this way then. What do you want to be remembered for?

Bracewell: You could rephrase it again and say, "What will I be remembered for?" I could make a guess at that but anything you wanted to be remembered for, you'd have a lot of luck if it worked out that way. I don't think you have much control over it. Well, some people do. Some people invent myths about themselves and plant stories, but most of us we don't have any control. I mean, look at any number of scientific things like the Fraunhofer lines. He's remembered for discovering the Fraunhofer lines. Pity they were discovered by Wollaston[50]. There are many cases like that. People are actually remembered. They didn't think they'd be remembered for that and probably couldn't care less, although I dare say if they knew,

[48] Pawsey, J.L., & Bracewell, R.N. (1955). *Radio Astronomy*. Oxford, Clarendon Press.

[49] Alfred Wegener (1880–1930). https://en.wikipedia.org/wiki/Alfred_Wegener

[50] W. H. Wollaston FRS (1766-–1828). https://en.wikipedia.org/wiki/William_Hyde_Wollaston

they'd be … Einstein's name will never be forgotten, nor will Archimedes, but other people who have done remarkable things are just completely forgotten. I don't worry too much about what I'm going to be remembered for.

Ronald Newbold Bracewell AO (22 July 1921–12 August 2007) was the Lewis M. Terman Professor of Electrical Engineering of the Space, Telecommunications, and Radioscience Laboratory at Stanford University.

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Reflections on Mozart

David Hush, FRSN

E-mail: david@hushedition.com

Abstract

This article seeks to explain how Mozart's music is unique. The article begins by positing a general view of Mozart's life achievement, drawing on Paul Johnson's short new biography for support. The article then proceeds to give examples of Mozart's originality, covering the solo instrumental, chamber, vocal and orchestral divisions of his œuvre.

What is it about Mozart's music that makes it so remarkable?[1] At the highest levels, it is not always constructive to compare the relative merits of different composers. However, there are times when it is useful to cite the work of another composer by way of reference.

When Beethoven started writing, he produced works that stand out for two principal reasons: the tremendous beauty of sound and a certain spiritual simplicity. As Beethoven grew older, he became more complex, and as he became more complex he became more introspective.

I do not for a moment wish to suggest that in the later works of Beethoven we no longer hear a beauty of sound; on the contrary, this special quality never deserted him. In the later works, we hear a different kind of beauty from that which distinguishes his earlier works. Moreover, the simplicity of spirit that one finds in the early Beethoven is nowhere near as manifest in the later works.

With Mozart, it's a different story. In the early Mozart, one hears an almost overwhelming beauty of sound. In addition, we encounter a simplicity of spirit, perhaps even more so than with early Beethoven, though this particular opinion remains subjective. The most important point about Mozart is that the profound beauty of sound and striking simplicity of spirit that one hears in the early works remained with him all his life.

I think it is fair to posit that of all the composers in the Western canon Mozart stands out as having written music that is the most beautiful. However, the fact remains that his music is so compellingly melodic and mellifluous as to belie the remarkable technical mastery lying behind it.

If we were to attempt the audacity to sum up Mozart's overall achievement in just a few words, it might be that throughout his magnificent, multi-faceted output he wrote music that radiates the dance of life to an uncommon degree.

One of the most popular works in the orchestral repertory is the *Nutcracker Suite* by Tchaikovsky. For all that this is a wonderful piece, there is no substitute for seeing the complete ballet with all the dancers up there on the stage.

With the music of Wolfgang Amadeus, by contrast, we can hear the dance of life without resorting to, or feeling any necessity to visualise, a choreographed ballet on the stage.

[1] The whole of Mozart's output is available online. http://dme.mozarteum.at/DME/nma/start.php?l=2

Such is Mozart's genius that regardless of whether we are listening to a solo piece, a chamber work featuring just a small number of instruments, or a much larger work for solo instrument and orchestra, as if by magic we hear the dance of life unfolding before our very ears.

In point of fact, Mozart was an inveterate and indefatigable dancer throughout his life — indeed virtually to his deathbed.

It is instructive to see how Mozart's infatuation with the dance manifests itself in larger compositions. Mozart's first collaboration with Lorenzo Da Ponte was *The Marriage of Figaro*, universally regarded as one of his best operas.

As Paul Johnson points out in his excellent new short book on Mozart, the play by Beaumarchais on which it is based "was a consciously radical assault on aristocratic privileges and pretensions and ran into trouble everywhere for precisely the reason that it showed humble-born persons as morally superior to aristocrats and getting the better of them for that reason — having higher intelligence, too. That is what initially attracted Mozart so strongly to the project, for it gave him its emotional dynamism" (Johnson, 2014, p. 90).

Without question, Beaumarchais' original scenario held a special resonance for Mozart in the composer's professional life. We learn from Johnson how Mozart and Da Ponte "first unconsciously, then quite deliberately and systematically, transformed the play into a comic epic of forgiveness, reconciliation, and final delight. Score settling became peace with honor, and revenge melted into content" (Johnson, 2014, p. 91).

> *Figaro* is thus the embodiment of Mozart's emotional nature in music. He was a fundamentally easygoing person, whose brief spasms of hot temper and outbursts of grievances were mere cloudlets racing across a sunny view of life. He enjoyed existence and wanted everyone to be as happy as he. He believed they could be, too, if only they were sensible. *Figaro*, in the end, shows everyone more or less being sensible, decent, and forgiving — and so happy. That is why it is not only Mozart's best opera but the one people love, probably more loved than any other in the repertoire. (Johnson, 2014, p. 91)

The transformation of Beaumachais' dramatic parable into a more universally accessible and loveable opera buffa lends credence to the view that in his stage works Mozart subscribed, consciously or not, to a world vision characterised by a certain playfulness and *joie de vivre* or, if you will, a dance of life.

With the piano sonatas, chamber works, instrumental concertos and symphonies we have no recourse to an external program or narrative. In other words, we enter the realm of absolute music. When we listen to Mozart's instrumental music, we are able to hear notes dancing in a multitude of configurations and permutations. In this respect, Mozart was a choreographer of genius.

In order to hear Mozart's dance of life encapsulated at the highest levels of sublimation, we need to listen to those interpretative artists who by virtue of talent, temperament and intuition are completely attuned to the rhythm of his musical breathing. The names that spring most readily to mind are Clara Haskil for the piano works, Arthur Grumiaux for the violin works, the Amadeus Quartet for the string quartets and quintets, and Bruno Walter for the symphonies and operas.

There would be much to support Johnson's contention that in writing both operas and sacred choral music Mozart "lived in two quite distinct universes, which he kept entirely separate, intellectually and emotionally."

> It is part of Mozart's genius that he could dwell simultaneously, without any sense of discomfort or uneasiness, on two quite different planes of sensibility — rather as he could switch from a carom in billiards to write five bars of a string quartet, then back again, without trouble. (Johnson, 2014, p. 101)

Yet by Johnson's own admission there is a discernible connection between *Figaro* and the Requiem Mass, Mozart's greatest sacred work. The net result is one of the most deeply personal compositions Mozart ever wrote. While serious, it is never solemn or austere. As Johnson points out, there is no hint of despair in the entire work.

> On the contrary, there is a consistent note of gentleness, love, reconciliation, and peace. This is epitomized by the Confutatis, which implies eventual admission to paradise. In a curious way, the atmosphere of the Requiem is the spiritual equivalent to the spirit of forgiveness and acceptance we find in the last act of *Figaro* … Mozart knew it would all come out right in the end. It is hard to imagine two works more different than *Figaro* and the Requiem, yet they both breathe this message, the theme, in some ways, of his life: "Never despair." (Johnson, 2014, p. 131)

Mozart had a remarkable ability to intuit the harmonic ramifications of a single chord. A fine example is the second movement of String Quartet No. 7 in E-flat, KV 160.[2] This movement is a sonata form in A-flat.

Instead of starting on the tonic, this movement opens with a seventh chord based on the submediant. The chord resolves to the supertonic on the second beat of the bar.

In the second bar, Mozart gives the dominant seventh which resolves to the tonic on the second beat of the bar. This version of the tonic, however, has the note C sounding in the first violin; no fewer than three more bars elapse before the harmonic status of the tonic is clearly established.

Mozart's ability to approach the tonic in such an unusual way at the start of a new movement shows that he was already an original.

Mozart was all of 17 when he wrote this piece.

The first movement of the Seventh Quartet displays a different kind of originality with respect to harmony. It is a sonata form in E-flat.

In the exposition, there is a 4-bar passage leading up to the second subject in B-flat.

In the recapitulation, bars 71–74 constitute a 4-bar passage leading up to the second subject, now transposed to the key of the tonic.

With the sole exception of an alteration to the violins' figuration on the last beat of bar 72, these bars are identical to the corresponding 4-bar passage in the exposition.

Such a procedure gives the lie to the rule of thumb that for a sonata movement to be successful the second subject of the recapitulation must be approached via a different harmonic trajectory from its homologue in the exposition.

[2] KV is an abbreviation in German for *Köchel Verzeichnis*. It is a register for all the compositions of Wolfgang Amadeus Mozart (1756–1791).

An examination of Mozart's œuvre confirms that he used this procedure more than once. A similar process may be observed in the first movement of Piano Sonata No. 2 in F, KV 280.

Mozart's originality with respect to harmony carries over to the realm of large-scale form.

The slow movement of the "Jupiter" Symphony No. 41, KV 551, is a sonata form in F.

The coda of this movement is based on the first subject. While the first subject was recognisable at the start of the recapitulation, the version of the subject in the coda bears a much closer resemblance to its homologue as it was initially heard in the exposition.

This particular application of sonata form is original.

Beethoven is quite rightly credited for his genius in incorporating fugues into several late multi-movement compositions: Piano Sonata No. 29 "Hammerklavier" in B-flat, Op. 106 (1818); Piano Sonata No. 31 in A-flat, Op. 110 (1821); and String Quartet No. 14 in C-sharp minor, Op. 131 (1826).

But the fact remains that Mozart, at a much younger age, wrote more than one string quartet which incorporated a fully-fledged fugue. Cases in point are the last movements of the following: String Quartet No. 8 in F, KV 168 (1773); String Quartet No. 13 in D minor, KV 173 (1773).

While the range of Mozart's songs cannot compare to that of Schubert's, it should be remembered that the latter composer's coming of age coincided with a late blooming of German verse, without which Schubert would have had recourse to a much narrower range of texts. It is reasonable to posit, moreover, that unless we count a few arias by Bach only Mozart shared Schubert's innate gift in writing for the solo voice.

It is hoped that the above citations from Mozart's output, while few in number, will lend support to the view of the composer as one with an exceptionally high degree of originality, with a musical intuition second to none.

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Reference

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Global challenges: personal reflections on the Stockholm "New Shape" competition

Len Fisher

School of Physics, University of Bristol, Bristol, UK

E-mail: len.fisher@bristol.ac.uk

Abstract

The Stockholm-based Global Challenges "New Shape" competition, which attracted 2,702 entrants from 122 countries, aimed to promote new ideas for the governance of global catastrophic risks. Here I tell the story of my role as one of 14 eventual finalists. It is a story of ideas — ideas that formed the background, ideas that emerged in the course of the finals, and ideas about how we might take things forward in the future. As Sir William Bragg put it in his famous introduction to *The Double Helix* (Watson 1967), this is not a history, but an autobiographical contribution to the history that may someday be written.

Background

The world received a sharp rebuke with the release of the Intergovernmental Panel on Climate Change (IPCC) Special Report on Global Warming (IPCC 2018). Mankind's use of fossil fuels was particularly targeted, with the recommendation that their use be completely phased out by 2050 if average global temperature rises of 3°C to 4°C, or even more, are to be avoided.

But climate change is not the only threat to mankind's future well-being and security. Table 1 is a list of other threats, compiled from three recent sources: the *World Economic Forum Global Risks Report* (WEF 2018); the *Global Challenges Report on Global Risks* (GCF 2018); and Julian Cribb's *Surviving the 21st Century* (Cribb 2017).

Table 1

1) Degrading environment and resource depletion (WEF; JC)
2) Ecological collapse (GCF; JC)
3) Food insecurity (JC)
4) Pandemics (GCF; JC)
5) Population and urban expansion (WEF; JC)
6) Rising geographic mobility (WEF)
7) Changing landscape of international governance (WEF)
8) Artificial intelligence and rising cyber dependency (WEF; JC)
9) Increasing national sentiment (WEF)
10) Increasing polarization of societies (WEF)
11) Shifting power (WEF)
12) Rising income and wealth disparity (WEF)
13) Ageing population WEF)
14) Growing middle class in emerging economies[1] (WEF)
15) Rising chronic disease (WEF)
16) Weapons of mass destruction (GCF; JC)
17) Asteroid impact (GCF)
18) Supervolcanic eruption[2] (GCF)
19) Solar geoengineering (GCF)
20) Our capacity for self-delusion (JC)

[1] According to *The Kyoto Manifesto for Global Economics* (Yamash'ta et al. 2018), this threat should be subsumed under the broader threat of "current economic paradigms."

[2] My colleague Russell Blong has pointed out that the risk from superflares generated by geomagnetic storms seems to be at least as serious as that posed by supervolcanic eruptions (Lingam & Loeb 2017).

That's quite a list, and one that reflects the preoccupations of the different organizations and authors. But the major question remains the same in all cases: how can human society cope?

One way to cope is to develop methods by which approaching tipping points can be predicted, and avoided or prepared for. This was an approach particularly developed by Marten Scheffer and his colleagues in the first decade of this century (Scheffer 2009; Scheffer et al. 2009). It was a major stimulus for my 2011 book *Crashes, Crises and Calamities: How We Can Use Science to Read the Early-Warning Signs*.

But there was a problem. The warning signs, which concern gross changes in economies, societies or ecosystems, usually became obvious only when it was too late to do anything about the emerging situation. One example is the collapse of Lehman Brothers in 2008, which was preceded by an increase in a mathematical indicator known as *information dissipation length* (Quax et al. 2013), but where the this measure was only obvious in retrospect. A more general warning sign identified by Scheffer and his colleagues is *critical slowing down*, where a system takes longer than usual to recover from small perturbations and disruptions. This latter sign is very general, and is associated with other signals, such as increasingly large swings between extremes. However, as the history of actions to cope with climate change has shown, it is difficult to persuade policy-makers to take such signals seriously, even when they become blindingly obvious to scientists. Critical slowing down and its associated signals are also not so useful for financial markets because, as Scheffer et al. have pointed out, once a warning sign is known, its effectiveness becomes diminished as people start to use it to make a profit out of the situation.

There was also another problem — one whose significance has only become apparent in the last decade, and whose recognition sparked my entry into the Global Challenges competition. It is the problem of interconnectedness between different threats.

Emerging global threats continue to be treated as separate and independent entities, often by different bodies. The IPCC, for example, does not concern itself with environmental degradation, such as the increasing amount of plastic in our oceans (Eriksen et al. 2014), let alone the alarming increase in antibiotic resistance (Zaman et al. 2017) that does not appear in any of the lists, although it is mentioned in the body of the WEF report. On the other hand, those who address the dangers of rising cyber dependency (Helbing et al. 2017), first brought to public attention by Nick Bostrom (2014), tend to focus on its effect on governance and social issues, while seldom considering its contribution to environmental degradation or its potential contribution to global warming (Jardin 2017).

The problem with treating global threats in this way is that many of them are interconnected, sometimes in complex ways. Global warming, for example, is already affecting food security, with longer growing seasons meaning that pests can survive from one season to the next (FAO 2008). Our choice of food may also affect global warming. Marco Springmann from the Oxford Martin School pointed out in a BBC radio interview (Springmann 2018) that "the food system is a major driver of climate change — it emits about a quarter of all greenhouse gas emissions." This may shrink if enough of us switch to a more vegetable-based diet.

Rising geographic mobility and increasing national sentiment are obviously at odds with each other in many countries, some of which continue to promote coal as an energy source, even though it is a major contributor to global warming (IPCC 2018). But phasing out its use will affect jobs, and also energy costs in the short term, with consequential social disruption, and perhaps changes of government. This in turn can affect biodiversity, especially with just five countries — Russia, Canada, the United States, Australia and Brazil — holding 70% of the remaining wilderness (Watson et al. 2018). Witness the situation in Haiti, where 99% of primary forest has been lost (Hedges et al. 2018), and Brazil, where the new president is a supporter of expanded agribusiness and concomitant rainforest destruction (*Nature* editorial 2018). With loss of habitat, insects may be lost, including some that act as pollinators for crops (Winfree et al. 2011). Yet the latest UN Biodiversity Conference (UN Biodiversity Conference 2018) does not appear to have agriculture on the agenda. Nor did the World Health Summit (WHS 2018), concerned particularly with the global threat of pandemics, include the health of our planet's systems on its agenda.

The Threat of Interconnectedness

It has long been recognised that unexpected, and sometimes unpredictable knock-on effects may occur in interconnected systems. The psychologist Robert Merton had a name for it: "the law of unintended consequences" (Merton 1936).

One example is the story of what occurred when a crisis resolution and home treatment scheme was introduced into the Welsh mental health system as an alternative to hospital admission (Hannigan 2013). The intentions were good, but "Participants described parts of the interconnected system being closed to release resources, staff gravitating to new crisis services leaving holes elsewhere, and the most needy service users being cared for by the least experienced workers."

Another nice example is the history of Viagra® (Sildenafil). Originally intended as a treatment for hypertension and angina pectoris, it was found instead to increase erectile function in men (Ban 2006). Accounts vary as to how this effect was noticed; according to a pharmacological colleague, clinicians at the Morriston Hospital in Swansea began to wonder why male patients in a trial were not returning the excess pills after completion of the trial.

On a more contemporary note, "voluntourism" (where volunteers from richer countries pay to do charity work in poorer countries) is having unintended knock-on effects that include neglect of locals' desires, hindering of work progress, loss of local jobs, rationalizations of poverty (Guttentag 2009), child trafficking, and the unnecessary placement of children in orphanages (Martin & Katie 2014).

The additional threat posed by interconnection between different global risks was brought to public and scientific attention by Dirk Helbing in a seminal *Nature* article in 2013. Helbing's prescient article has still not had sufficient impact, although some authors have noted the possibility of interconnection between global threats. Cribb (2017), for example, specifically refers to his list of risks as "intersecting," while Short et al. (2018) speak of the "changing population demographics, antibiotic resistance and climate change, which we will face in the context of any future influenza virus pandemic." Resilience consultant Roland Kupers (2018) offers a specific example: "In

a deeply interconnected world, stresses and shocks propagate across systems in ways that evade forecasting. Climate change is linked to the Syrian civil war, which is connected to heightened concern over immigration, which precipitated Brexit."

Other authors have examined the possibility of dealing with interconnected threats by means of "risk trade-offs". Baum & Barrett (2017), for example, offer "integrated assessment … to put all of the global catastrophic risks into one analysis in order to perform cross-risk evaluation and inform risk-risk trade-offs and allocation prioritization."

But none of the above authors addresses the most important point of all: the one that I was anxious to address in my Global Challenges entry. This is that complex adaptive networks can have their own ways of doing things — ways that are not predictable from the behaviours of the individual members, and which are not always in the best interests of those members (Reyers et al. 2018). Connectivity in a network "may lead to emergent behaviour whereby local interactions lead to self-organised phenomena observable at larger spatial scales that cannot be predicted (or at least they are not obvious at the local level: what Bedau (1997) calls "weak emergence")" (Turnbull et al. (2018).

Increasing interconnection, often seen to be a good thing in today's increasingly networked world, can sometimes lead to sudden and dramatic collapse. The fall of the Roman Empire offers a spectacular example, as pointed out by the American historian Joseph Tainter (1988). Prior to Tainter's work, historians had commonly interpreted the collapse of societies and civilizations in terms of a cyclical view of history — the idea that civilizations have a natural growth and decay cycle, with collapse having its seeds in the distant past (e.g. Gibbon 1776–1788).

This model, where "the wheel of history revolves slowly, like an old water wheel in summer" and civilizations "cycle sedately from Arcadia to Apogee to Armageddon" (Ferguson 2010) has entered the popular consciousness, and has been reinforced by such authors as Jared Diamond (2005), albeit with considerable professional criticism (McAnany & Yoffee 2009; Ferguson 2010). It was even used by science fiction author Isaac Asimov (1951) in his *Foundation* novels, which are based on the idea that the large-scale sweep of history can be predicted by mathematics.[3] Sometimes this might be possible (Lagi et al. 2011). But mathematics tells us a very different, and much less certain story than that supposed by Asimov and Diamond.

As Tainter pointed out in his pioneering book *The Collapse of Complex Societies* (1988), the Roman Empire collapsed very rapidly, the population of Rome dropping by 75% in just five decades (Ferguson 2010). He attributed this, not to remote historical circumstances, but to the fact that the empire had reached a level of [interconnected] complexity that rendered it very susceptible to small perturbations. "The process of collapse" he said "is a matter of rapid, substantial decline in an established level of complexity." An equivalent example in modern times is the sudden catastrophic failure of power grids that have reached a level of complexity that renders them vulnerable to small localized events (Jing et al. 2003; Andersson et al. 2005; Simpson-Porco et al. 2015).

[3] The concept is encapsulated in the fictional science of "psychohistory," which Asimov described in an interview (Asimov 1987) as "a science in which things could be predicted on a probabilistic or statistical basis".

What is complexity? In the popular mind it is often confused with chaos, but the two are antithetical. According to philosopher and complexity researcher Paul Cilliers (2000) "Complexity is about how a huge number of extremely complicated and dynamic sets of relationships can generate some very simple behavioral patterns, whereas chaotic behavior, in the sense of deterministic chaos, is the result of a relatively small number of non-linear interactions." So the simple action of a butterfly flapping its wings can lead to chaotic storms, whereas the enormously complicated and interacting chemical reactions in the cells of our body can produce relatively simple behaviour patterns like walking.

There are many practical examples of complexity in everyday life. The Santa Fe Institute, set up in 1984 to study their consequences (German, undated), provides a substantial list in its manifesto (SFI undated): "Complexity arises in any system in which many agents interact and adapt to one another and their environments. Examples of these complex systems include the nervous system, the Internet, ecosystems, economies, cities, and civilizations. As individual agents interact and adapt within these systems [in which case they are called *complex adaptive systems*], evolutionary processes and often surprising `emergent' behaviors arise at the macro level."

We are far from understanding, let alone predicting, emergent behaviours in complex adaptive systems (Turnbull et al. 2018). Sometimes these can lead to patterns that are stable over long periods of time, as happens with many biological organisms (Kitano 2002). But biological organisms have the advantage of evolutionary tuning, with unfavourable networks falling by the wayside. Long-term stability is the exception, rather than the rule, when it comes to complex adaptive networks. The reasons for this lie deep in the mathematics of such networks (May 1972, 1976), but one thing is now clear — *all complex adaptive networks contain within themselves the possibility of sudden ("critical") transition of the whole system to a new and different state* (Scheffer 2009; Reyers et al. 2018).

In other words, when it comes to the interconnected complex adaptive network of global risks, collapse is always on the cards.

Systemic Collapse

I had already written a book about complexity in everyday life (Fisher 2009), but it was only in the ensuing years that I realized how systemic collapse can happen at any time in a complex adaptive system, sometimes with little or no warning. This feature of our global economic, ecological and social networks became central to a meeting between scientists, politicians and policy-makers that was held in Venice in 2012.

I was lucky enough to be invited to attend the meeting, and eventually to be invited to write the final report (Fisher 2013). In it I focused on slowly developing catastrophic risks: those where slow and imperceptible changes may bring us to the brink of catastrophe without our even realizing it. The process is sometimes viewed in terms of Bak's "sand-pile" model (Bak 1999), where grains of sand are added one at a time to a pile until the addition of just one more grain initiates a cascade of collapse.

Bak's model is an idealized scenario, and does not necessarily describe the more complex events that may precipitate sudden collapse in real-life "sand-piles," such as colliery spoil heaps (Van Burkalow 1945; Aalto et al. 1997). But one feature that Bak's model and real-life collapses share is that collapse may

happen at any scale. Sometimes there may be a little trickle of sand down the side of the pile. At other times there will be a virtual avalanche, with the whole pile collapsing. Before that final, fatal grain is dropped, however, there seems to be no way of knowing what the scale of the collapse will be.

Thus it was with the global financial collapse of 2008 (Crotty 2009, Marks 2015). Banks have collapsed before, sometimes with no more than local damage (the trickle down the side of the sandpile). But when Lehmann Brothers filed for bankruptcy on September 15, 2008, the event initiated the collapse of the whole global financial sandpile (Dimitriou et al. 2013).

The collapse of a sandpile provides a dramatic image of systemic collapse, and even some relevant mathematics, but I was beginning to realize that it does not give a picture of what actually happens within the system. For this, we must turn to network science, and picture the system as a web — not a stationary web, but a dynamic one, where the nature and strength of the connections are continually evolving. This is the picture that Andy Haldane, chief economist for the Bank of England, used when he analysed the underlying reasons for systemic financial collapse in a speech delivered to the Peterson Institute for International Economics (Haldane 2017):

> the behaviour of complex, interconnected financial systems can be very sensitive to small changes in initial conditions and shocks.… Complex systems exhibit tipping points, with small changes in parameter values capable of moving the system from stability to collapse… The radical uncertainty in such complex webs generates emergent behaviour which can be near-impossible to predict, model and estimate.

This is certainly the case for the complex socio-economic-ecological web to which we all belong. The serious global challenges with which we are now faced can interact in complex and unpredictable ways, to produce complex and unpredictable outcomes. It is this situation that we must learn to manage (Liu et al. 2015).

It is a web without a spider, as the following figure from the World Economic Forum (WEF 2018) demonstrates (see figure opposite).

How are we to maintain stability, or cope with sudden, often catastrophic change in such a web? Should we attempt to introduce a spider, in the form of some over-arching governing body, to try to control the tensions and stability of all of the strands? But maybe this would lead to new instabilities, and a new "equilibrium." Is there, perhaps, some other way?

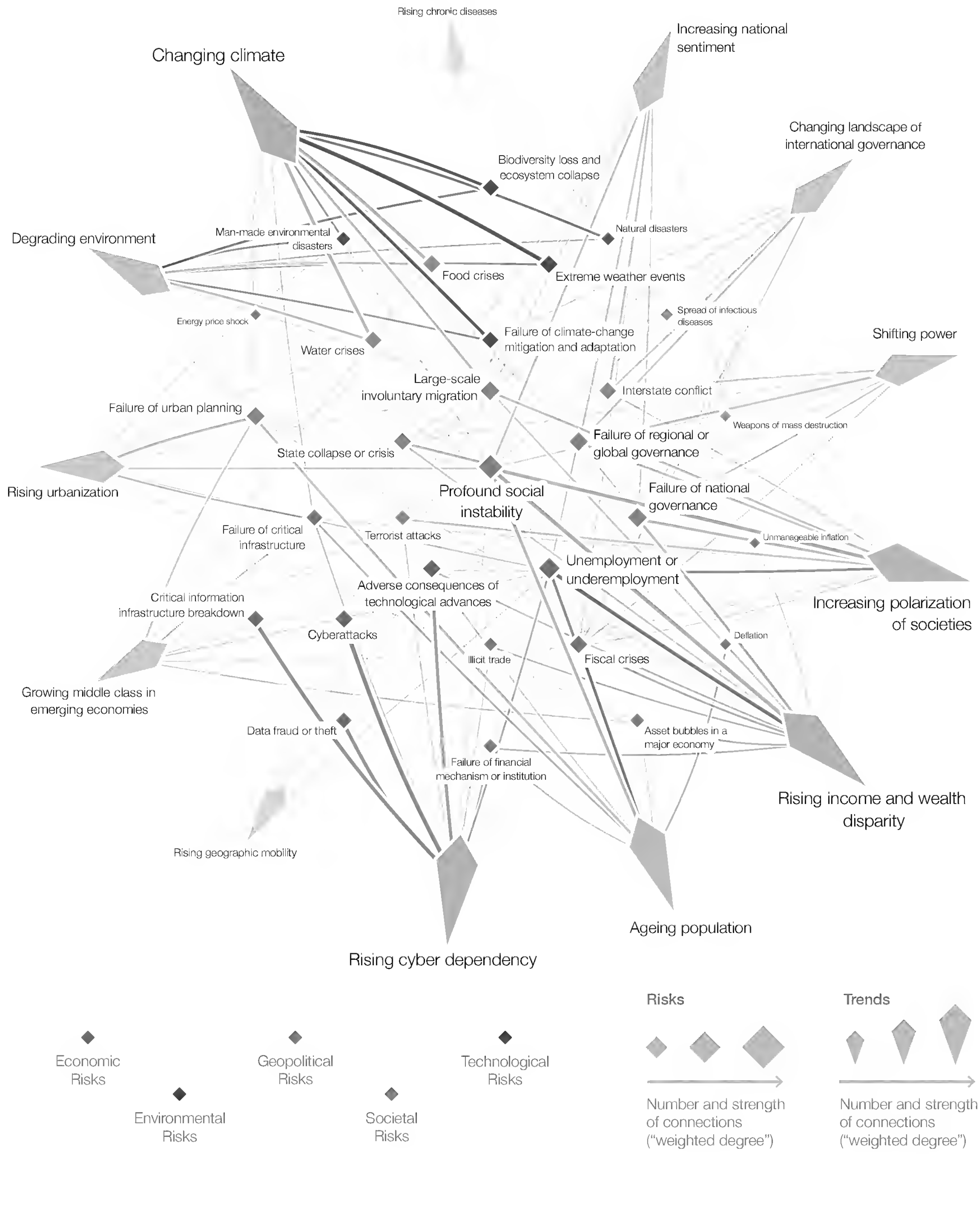
Three Problems

I had now realized that there are three factors that we have to cope with when it comes to the governance of complex adaptive systems:

1) Actions in one part of the network may have unintended consequences (positive or negative) for other parts
2) Small perturbations may transmit rapidly through a network, sometimes causing disruption and collapse
3) The network as a whole may "flip" to some quite different state with *no identifiable cause* and with little or no warning

Present systems of governance are simply too cumbersome, and too unaware of the underlying problems associated with real-world complex adaptive networks, to be

Figure II: The Risks-Trends Interconnections Map 2018



Source: World Economic Forum Global Risks Perception Survey 2017–2018.
Note: Survey respondents were asked to select the three trends that are the most important in shaping global development in the next 10 years. For each of the three trends identified, respondents were asked to select the risks that are most strongly driven by those trends. See Appendix B for more details. To ensure legibility, the names of the global risks are abbreviated; see Appendix A for the full name and description.

effective in coping with such rapidly occurring, and often unexpected eventualities. As Liu et al. (2015) and others have cogently argued, we need up-to-date complex network thinking if we are to make any real progress.

Enter the Global Challenges Foundation

In November 2016 an international competition was announced "to help incite bold and visionary ideas to tackle global risks" (GCF 2017). The competition was the brainchild of Laszlo Szombatfalvy, a Hungarian-born Swedish financial analyst who was determined to use his money to help the world avoid nuclear and other catastrophes. Szombatfalvy established the non-political *Global Challenges Foundation*, and it was through this foundation that a potential prize of $US5m was offered for "improved frameworks of global catastrophic risks".

The foundation had its own list of potential global threats, most of which are given in the table at the beginning of this article. But there was one more — one that particularly attracted my attention. This was a category called "unknown risks."

The concept is well known from former U.S. Secretary of Defense Donald Rumsfeld's response to a question at a news briefing. The question concerned the lack of evidence linking the government of Iraq with the supply of weapons of mass destruction to terrorist groups. As part of his response (US Department of Defense 2002), Rumsfeld famously said: "there are known knowns; there are things we know we know. We also know there are known unknowns; that is to say we know there are some things we do not know. But there are also unknown unknowns — the ones we don't know we don't know."

By now I had realized that the biggest unknown of all with respect to global challenges is the risk that some combination of knock-on effects involving different risks could drive collapse of the whole shimmering web. To researchers in the field, this is a "known unknown". Sadly, so far as most government institutions and politicians are concerned, it remains an "unknown unknown".

When the Global Challenges *New Shape* competition opened, I had already been working for some time with my colleague Anders Sandberg at Oxford University's Institute for the Future of Humanity to try to understand how we might develop new approaches to the governance of this particular known unknown. We had met at a meeting of the International Risk Governance Council in Zurich, and found that we were thinking along similar lines, so decided to combine our resources. But only when the competition was near closing in September 2017 did an idea occur to me that might provide the basis for a possible solution. I rapidly wrote a proposal (Fisher 2017a), and made it just before the deadline.

I proposed "a global insurance and reinsurance model. A new institution receives premiums paid by member countries, in order to insure them against the effects of global catastrophic risks. The institution provides expert advice to the member countries, and financial backing for investments in cooperative projects to achieve risk reduction. Concentrating on a financial task would keep the institution flexible and independent. Decisions would be based on the principles of 'effective altruism,' with decisions constantly monitored by AI, Big Data and statistical analysis to improve the institution's priorities and performance. The submission suggests

a pilot project being instituted by a smaller number of countries, in order for a first evaluation and improvement round."

The key words here were "flexible and independent." Flexible, because the threats from networked risks can emerge suddenly, with little or no warning, and require fast and appropriate responses to deal with them. Independent, because the internal politics of global human institutions such as the UN lead to ponderous processes that are not fit for purpose.[4]

I was not the only one to propose a fresh approach to tackling global catastrophic risk. When the competition closed, the organizers found themselves with 2,702 entries, from individuals or groups across 122 countries. A series of international committees whittled these down to 68 semi-finalists, and from these just 14 (out of a possible maximum of 20) were selected to appear in Stockholm in May 2018 to face the finals judges.

Much to my surprise, I was one of them.

The "New Shape" Competition

The finals entries (GCF 2018A) could be divided into two broad categories — those that sought evolution, and those that sought revolution. Most of them, including mine, particularly sought to find *fairer* approaches to international governance (New Shape library 2018; New Shape summaries 2018).

Certainly we need to find some way forward that avoids the power struggles that bedevil the United Nations and many other international organizations concerned with global catastrophes, potential and actual. But we also need to find fresh and *effective* ways to tackle global threats. This means facing up to the underlying problems.

I was (and still am) of the opinion that the most serious problem of all is that of interconnection between threats. I failed to get this point over to the judges, either in my submission or in my final address. In truth, I had failed to get it over to myself. Only through interaction with the other finalists, discussions with colleagues, and further consideration of the points raised, have I been able to bring it to a proper focus.

All of the finalists had interviews with the judging panel, and then just five minutes in front of a live audience (including the judges) to make their main point. It was probably the first and only time in our lives that we were giving talks worth a potential one million US dollars per minute.

The three eventual winners (GCF 2018B) were those who best convinced the judges that they had a practical approach to improving fairness in decision-making (see Box overleaf).

[4] Stephen Hill, FRSN, a former Regional Director of UNESCO, adds a caveat that "At HQ level relations with other Agencies are in self-contained silos and direct relations only occur at the very top (above the 'knowledge-band') + staff are too often embedded in their own internal bureaucratic process and rewards. The Field is however very different. … the 'Country Team' — heads of all Agencies — meet at least weekly and work in close cooperation."

Brief Extracts from Summaries of Winning Proposals
(see GCF 2018A for more detail)

1. A New Shape: helping the UN to do itself out of a job — Natalie Samarasinghe

"The model proposed brings businesses, Non-Governmental Organizations (NGOs) and young people into UN governance structures, taking the International Labour Organization (ILO) as a starting point. Concurrently, the UN would transfer its development-related tasks to these stakeholders, who would bid competitively for contracts."

2. Global Governance and the Emergence of Global Institutions for the 21st Century — Augusto Lopez-Claros, Arthur Dahl & Maja Groff

"The submission proposes a revised United Nations Charter, instituting a reformed UN General Assembly directly elected by popular vote and a second civil society-focused chamber. ….. A new Bill of Rights is to prescribe the parameters for UN action, and the global human rights will be upheld … by an International Human Rights Tribunal. A new funding mechanism would link members' indirect tax revenues to the UN budget in a fixed proportion."

3. AI-supported global governance through bottom-up deliberation — Soushiant Zanganehpour

"… this proposal suggests combining a blockchain-based global identity system with an AI-based collaboration platform to fuel citizen collaboration and ideation around policies and budget suggestions, as the entry point for decentralized citizen participation in governance. … The technology supports the creation of new local and global institutions to help create relevant and pragmatic solutions for stewarding local, national, and global commons."

All are excellent in their way, and fitted the criteria of fairness and implementability. But this has just been a first step. Now it is time for the next step; that of adapting the suggested approaches (not necessarily just those of the winners) for *effectiveness* in the face of the dangers of networked catastrophic risks.

Whatever the approach may be, it has to be capable of:

1) Watching for warning signs in time to take evasive action (for example, reducing carbon emissions to avoid the worst of anthropogenic global warming).
2) Flexibility to act fast and decisively when catastrophic change threatens.
3) Deciding *in advance* on the balance of investment between trying to maintain the *status quo* or adapting to new circumstances after a critical transition has occurred.

Certainly the United Nations, a spider with 15 legs,[5] needs reform, being too slow and too unbalanced. It is also fair to say that reform may not be enough, and that a revolutionary approach to global governance may be required to establish both flexibility and fairness in the face of global catastrophic risk.

This is especially so because of the obvious disparity between the time between when warning signs become sufficiently clear to be heeded, and the time-scales over which most current human institutions are able to make decisions and take effective action (Fisher 2011). This disparity is accentuated when cooperation between self-interested individuals, organizations or nations is required. The difficulties are described, but

[5] The UN has 15 specialized agencies, *none* of which has a specific remit to address global catastrophic risks.

unfortunately not resolved, by the insights of game theory, which concludes that "through following the logic of self-interest, they have somehow landed everyone in a position where self-interest is the last thing that is being served" (Fisher 2008).

My proposal of an insurance-based approach (Fisher 2017b) went some way towards addressing these issues, although the idea of dumping a large chunk of the United Nations and replacing it with a flexible and responsive insurance company did not find favour —partly because the image of an insurance company suggests a profit-making institution, even though I was at pains to emphasize that this was not what I was proposing.

I am still of the opinion, however, that some form of insurance-based thinking is the best way to cope with interconnected global threats. I suggested as much in an article for actuaries (Fisher 2017c), where I argued that "the insurance industry, and its actuarial practitioners, [should adopt] a *proactive* rather than a *reactive* approach" in helping society to deal with networked systemic risk.

I pointed out to an assembly of industry CEOs (Fisher 2017d) that the insurance (and, especially, the reinsurance) industry as it currently exists can help to deal with global systemic risks by adjusting premiums to favour customers that are taking positive action to help reduce the risks. In fact, if it does not do so, the industry itself is liable to collapse when disaster strikes.

We cannot, however, rely on the insurance industry *per se* to help plan for social justice, or even social survival, in the face of imminent catastrophe. For this we need to introduce a new way of thinking into existing or new social governance institutions — one that is based on *understanding* and *evidence* rather than power and profit (Fisher 2018).

Many of the entries in the Global Challenges New Shape competition suggested structures where new ways of thinking could be made possible. What is needed now is for concrete proposals as to how such structures could be adapted to face the real questions posed by interconnected global threats. Perhaps evolution is possible. Perhaps revolution is the best way forward. But find a way forward we must, because the threats are already upon us. It is only a matter of time, and we do not know how much of that we have.

Where Do We Go From Here?

A new or modified institutional framework must be capable of making fast decisions, and taking fast and effective action in response to three major problems: the predictability of individual risks; the unpredictability of interlinked risks; and the decision of whether to invest in maintaining the *status quo* or adapting to changed circumstances.

Problem 1: Predictability of Individual Risks

The first and obvious problem is to deal with threats whose consequences are predictable.

The success of the Montreal protocol (Rae 2012) shows that it *can* be done. When the threat of the ozone hole became apparent thirty-odd years ago, nations cooperated *via* the protocol to phase out CFCs and other ozone-depleting substances.

The protocol generated cooperation through networking. It set compliance targets, and provided advice and resources to help developing countries meet those targets. It also had the proviso that signatories should not trade with non-signatories. That made

it very tough for smaller countries not to sign up, once the big countries had agreed to cooperate.

The keys to the success of the protocol were that:

i) the evidence had become compelling
ii) large and powerful nations recognized that there was an immediate threat that superseded national interests and required international cooperation to resolve
iii) alternatives to CFCs were available
iv) international cooperation was driven through a combination of carrot and stick

Why, then, did the Kyoto protocol for the reduction of carbon emissions fail?

According to Norwegian and German researchers (Hovi et al. 2010), it was because at least one powerful nation (the U.S.) did not meet condition ii), and said so in the famous (or infamous) Byrd-Hagel resolution of the U.S. Senate (Congress 1997–1998):

> that the United States should not be a signatory to any protocol … which would: (1) mandate new commitments to limit or reduce greenhouse gas emissions … unless the protocol or other agreement also mandates new specific scheduled commitments to limit or reduce greenhouse gas emissions for Developing Country Parties within the same compliance period; or (2) result in serious harm to the U.S. economy.

We are now in the murky area of game theory, where sectional interests (such as the coal industry in U.S. states, both Democrat and Republican) trumped global cooperation, to the long-term detriment of all concerned.[6] In fact, Hovi et al. invoke Putnam's (1988) two-level game theory as part of their explanation that political appearance mattered more than reality to US negotiators, Clinton and Gore.

Ultimately, coping even with individual risks requires convincing governments that there is an immediate threat that supersedes national or sectional interests. This is not an easy matter when questions of political power supervene, whether these questions involve cooperation between nations or the actions of supranational assemblies such as the UN.

Insurance-based thinking provides a possible solution. Insurance companies are concerned with defense against risk, and avoiding loss in the face of risk — just the sort of thinking that we need when faced with global catastrophic risk.

How that sort of thinking can be introduced into the international arena is a matter to be considered. But introduced it surely must be, especially in the light of:

Problem 2: Unpredictability of Interlinked Risks

According to analyst Flaviano Morone and his colleagues (Morone et al. 2018) "Collapses of dynamical systems into irrecoverable states are observed in ecosystems, human societies, financial systems and network infrastructures. *Despite their widespread occurrence and impact, these events remain largely unpredictable* [author's emphasis]".

It is worth quoting *in extenso* the comment in the recent review by Reyers et al. (2018), which should be required reading for politicians and policy-makers at all levels:

[6] It may be that even the Montreal Protocol is being damaged by sectional interests, since there is evidence that CFCs are still being manufactured and released by some countries in contravention of the protocol, although other explanations for the recent rise in atmospheric concentration are possible (Rehm 2018).

> SES [Social-Ecological Systems] research highlights that the properties, behaviors, and trajectories of complex SES cannot be determined by the microlevel social or ecological entities or subsystems and their properties alone. In SES, agents or entities interact, and from such interactions macrolevel patterns with new properties emerge, which then feed back on the system and influence the microlevel interactions of the agents … . This interplay between the adaptive responses of the entities and the emergent properties of the system implies that SES are more than the sum of the ecological or the social "parts." It is only through a focus at the macrolevel of emergent phenomena that explanations of things such as resilience as a system property, tipping points, the evolution of norms, or adaptive capacity, which are crucial to sustainable development, are offered. *Shifts to policy interventions, targets, and adaptive management, which acknowledge and are based on the system's irreducible complex structure, are proposed for sustaining desirable system outcomes* [author's emphasis].

A great deal of effort is now being put into understanding the role of connectivity in the sudden collapse of social-economic-ecological systems (Turnbull et al. 2018), but we are far from a complete understanding. Morone et al. (2018) believe that a topological invariant known as the *k*-core may hold the key, and understanding network behaviour from this perspective may be a useful guide to policy.

Sometimes we may be able to use this understanding to control the occurrence of a particular tipping point. This has been achieved in practice in relatively simple environments such as freshwater lakes (Pace et al. 2017). But the bald fact remains: in dealing with complex, interconnected threats, we must constantly be prepared for the risk of the unexpected. As Carl Folke and his colleagues from the Stockholm-based Resilience Alliance have argued (Folke et al. 2010), and as the World Economic Forum has also spelled out (WEF 2018), *resilience* is a major key to effective preparation.

Problem 3: Investment in Resilience

But what is resilience? There are more than 70 definitions in the literature (Fisher 2015). These vary between two extremes, with most trying to achieve a balance between the two (de Bruijn et al. 2010).

At one extreme, resilience is defined as the ability of a system to bounce back after stress, thus maintaining the *status quo*. This is the definition implicitly used by the World Economic Forum in its reports, and also by many authors concerned with environmental protection.

At the other extreme, resilience is seen as "the capacity of social–ecological systems to adapt or transform in response to unfamiliar, unexpected and extreme shocks" as proposed. This is the definition proposed by a group of distinguished scientists that includes ecologist Stephen Carpenter and the late economics Nobel laureate Kenneth Arrow (Carpenter et al. 2012).

Kupers (2018) names these two extremes respectively as "structural resilience" and "transformative resilience," and also identifies an "in-between" situation as "integrative resilience."

Unfortunately, most users of the term fail to specify what they mean by resilience. The Dutch hydrologist Ruben Dahm and his colleagues, for example (Dahm 2014), speak of the need "to increase [delta] cities' resilience to flooding". They propose several strategies,

including "developing urban infrastructure to decrease the effects of extreme rainfall" and "building in harmony with natural-systems dynamics". The first strategy is clearly aimed at maintaining the *status quo*, while the second is more concerned with adaptation in the face of the inevitable.

To recover, to adapt or to invest in both possibilities? Structural, transformative or integrative resilience? All make sense in the right context. We might want a city to recover from flooding, for example, or the world to adapt to the inevitable effects of climate change.

Long-term policies to promote either recovery or adaptation, or to prepare for both, are likely to be very different, and need to be put in place in advance.[7] This is where insurance-based thinking could come into its own, quantifying and continually updating the assessment of the relative risks, and preparing investment strategies accordingly.

It is a very different way of thinking from that currently in vogue among politicians, who are more concerned with offering (false) certainties based on dogma rather than reality. But the reality of interconnected threats, and of consequent sudden, society-wide global change is what we must face.

There is already some discussion within the industry about insuring against climate risk (Swann & Millar 2016; *Economist* 2018), with considerable evidence that the economic risks have been severely underestimated by the market (Stoerk et al. 2018). Just the name of the journal in which this last piece of work was published (*Review of Environmental Economics and Policy*) reflects the fact that policy-makers are still not taking account of the potentially serious interaction between environmental and other threats.

The insurance industry as present constituted does not provide an answer, since it is primarily concerned with short-term profits. Annual premiums don't encourage long-term thinking, and the industry in any case tends to be *reactive* rather than *proactive*. A pragmatic, insurance-based way of thinking about the interconnected risks of global catastrophic change, and preparing a balanced investment portfolio to cope with the changes is, however, surely a first, essential step for the governments, NGOs and inter-governmental organizations which are responsible in large measure for our future welfare.

Conclusion

The Global Challenges New Shape competition sought suggestions for new approaches to the governance of global threats. My approach was based on the premise that interconnection between global threats constitutes a particularly serious threat to humanity's future well-being, and even survival. Interconnection means that the threats form a complex adaptive network, with many, constantly changing, feedback

[7] Again, it is worth quoting *in extenso* from Reyers et al. (2018): "Social-ecological coevolution theories emphasize that diverse social and cultural contexts will shape, and be shaped by, diverse ecosystems in complex and continuous ways. The resultant diversity is the focus of much SES research, which emphasizes the importance of diversity in actors, ecosystems, institutions, and social-ecological interactions as sources of resilience. These sources create and enhance the novelty, knowledge, behavior, and strategies required to respond to shocks or ongoing change. *The relationship between diversity and resilience is, however, not linear* [author's emphasis]. Concepts such as response diversity, functional diversity, and redundancy are linked to tolerance of change, renewal and adaptation to change, as well as opening up pathways for transformation. Leslie & McCabe (2013) highlight the role of response diversity in human actions and decisions to the resilience of SES and thus to sustainable development in the Anthropocene."

loops. Such networks can collapse or change rapidly to a very different state through three mechanisms:

1) Unexpected consequences of small deliberate changes in some part of the network
2) Rapid transmission and amplification of small unplanned fluctuations in some part of the network
3) Unpredictable emergent behaviours of the network as a whole

Some progress has been made in identifying warning signs for imminent critical events, but most human institutions have been unable to respond effectively by the time that the warning signs become sufficiently obvious.

We thus need a new way of thinking; one that uses network science and forward planning to avoid critical transitions where possible, but which also has the capacity to make and implement rapid decisions when critical transitions become inevitable. Those decisions concern resilience, and the balance between investment in recovery after an event, or investment in adaptation to the new circumstances.

I believe that insurance-based thinking provides a possible solution. Insurance is concerned with defence against risk, and avoiding loss in the face of risk — just the sort of thinking that we need when faced with global catastrophic risk. This is not to suggest that traditional profit-oriented insurance companies should be involved — simply that the same style of thinking should now lie at the core of the governance structures responsible for our future safety and well-being in the face of global catastrophic risks.

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Thesis abstract

Essays in theoretical and experimental economics

Johannes Hoelzemann

Abstract of a thesis for a Doctorate of Philosophy submitted to University of New South Wales, Sydney, Australia

This thesis consists of three essays studying both theoretically and experimentally various aspects of coordination problems. Indeed, in this thesis we focus on dynamic games of information acquisition and transmission, organizational design and simple contract environments.

In the first chapter of this dissertation, we consider a dynamic public-good problem, where the public good in question is the *evolving information* about agents' common state of the world. Innovation and social learning are often the work of pioneers, who, by bearing the costs of experimenting with a new approach, create informational spill-overs for others. Whether we consider R&D, resource exploration, or the testing of a new drug, the information produced by a relatively small set of agents benefits a much larger group of agents. Indeed, R&D is universally recognized as an important factor of economic growth (Romer 1990; Grossman & Helpman 1993).[1] An economy's productivity level depends on innovation, which is driven by knowledge emerging from cumulative R&D experience as well as an economy's overall knowledge stock (Griliches 1988; Coe & Helpman 1995). Indeed, economic agents often endeavor to learn over time about some payoff-relevant aspect of their environment.

Think, for instance, of a pharmaceutical company conducting costly clinical trials to find out the effectiveness of a drug. Learning often requires a costly investment in information acquisition, so that agents face a dynamically evolving trade-off on how much information to acquire. Indeed, in light of the signals it receives, the pharmaceutical company will revise its beliefs and decide whether to incur the costs necessary to acquire additional information by continuing its trials, or to give up. It is thus important for economists to analyze pioneers' incentives for information production in the presence of informational spill-overs. Bandit problems involve the trade-off between exploration and exploitation. In their simplest form an agent has a choice between a safe arm with a known payoff or a risky arm whose likelihood of a payoff is unknown and can only be learned through experimentation. However, in the presence of information spillovers, agents can learn from others' experimentation. As a result, agents have an incentive to free-ride and experiment strategically through complex coordination. We implement such dynamic public-good problems in the experimental laboratory and find strong support for the prediction of free-riding because of strategic concerns. We also find strong evidence for behavior that is characteristic of Markov per-

[1] See the full thesis for the references — Ed.

fect equilibrium: non-cutoff behavior, lonely pioneers and frequent switches of action.

In the second chapter, we study organizational design and its role in coordination failure using the *network minimum game*: a (generalized) version of the minimum-effort game where dependencies between players are captured by a directed network. Indeed, organizations create patterns of coordinated activity. A key challenge in shaping these patterns is that fostering tacit coordination among large teams is difficult. An extensive experimental literature documents that coordination failure is almost inevitable in sufficiently large groups. Coordination problems in these studies are represented by the minimum-effort game (Harrison & Hirshleifer 1989; Van Huyck, Battalio, & Beil 1990), where players in a group aim to match the minimum action of the rest of the group. The minimum-effort game presents an extremely stylized view of organizational coordination, where each individual is held responsible for coordinating with everyone else. In practice, the scope and complexity of interactions within most organizations is limited—*by design*. Tasks are allocated and incentives are designed so that most individuals are responsible for coordinating only with a subset of coworkers. A subordinate executes instructions from his superior; production workers on the same assembly line coordinate with each other; a CEO is held responsible for the execution of his strategy by senior managers. In abstract terms, we take the perspective that organizational design specifies a network of interdependencies (in actions and payoffs) between individuals. Organization-wide coordination emerges from this ensemble of network interactions. We conduct a laboratory experiment to study how network structure influences coordination. Our experimental setting captures the notion of repeated interactions within long-lived organizations with persistent structure: subjects play in fixed groups, with fixed network structure, for ten rounds. Indeed, players are connected by an (exogenous) directed network, and each player has to match actions with his direct connections. We find that cycles of dependencies in the network minimum game are particularly destructive to coordination: coordination degrades if cycles are introduced into the network. Furthermore, our results highlight an interaction between network cycles and network density: coordination failure is most dramatic when the network is both dense and cyclic. Players consistently choose almost-maximal actions—regardless of connection density—on acyclic networks. In contrast, coordination is significantly more successful on sparse cyclic networks than on dense cyclic networks. So, density matters, but only for cyclic networks. Conversely, cycles matter, especially for dense networks. Indeed, the difference between the dense acyclic and dense cyclic network is a single dependency which, by completing cycles in the heretofore acyclic network, has catastrophic effects on overall coordination. Furthermore, acyclic networks make coordination resilient: initial coordination failure is often overcome after repeated play in acyclic networks, but not in cyclic networks. Our findings provide a novel perspective on the near-ubiquity of acyclic (e.g., hierarchical) structures in organizations.

In the final chapter, we study a simple contract environment with an ex-ante investment stage and where ex-post bargaining takes place under one-sided asymmetric information. There are two principal ways to organize economic activity: markets

and firms. Understanding the demarcation between the two has long occupied the attention of economists—at least since Coase (1937) famously asked why, if markets are an efficient means of allocating resources, do firms exist at all? Given that about half of all economic activity takes place in markets, and half in firms, it is perhaps not surprising that the study of the boundary between firms and markets has been an important topic for economists—indeed, giving rise to three Nobel prizes (Coase, Williamson, and Hart). Coase (1937) introduced the concept of transaction costs as a rationale for why using the price mechanism can be costly, and hence why transacting inside the firm may be preferable. In a series of contributions, Williamson (1971, 1975, 1979) unpacked the broad concept of transaction costs, emphasizing ex-post frictions such as haggling.

The modern theory of the firm—Property-Rights Theory—pioneered by Grossman & Hart (1986) and Hart & Moore (1990), emphasizes the ex-ante friction of under-investment. Specifically, parties anticipate renegotiation of their (incomplete) contract, and because only one party can hold residual control rights through asset ownership, the other party underinvests in the relationship. Recent theoretical work in PRT has moved toward emphasizing ex-post frictions, however, through the introduction of behavioral ingredients, in particular "reference points" and "aggrievement". This is quite clearly an attempt to formally model haggling costs and to allow asset ownership to play a role. Even though it is relatively early days, this approach has proved quite fruitful.

To an economist, however, perhaps the most natural haggling cost arises from bargaining under *asymmetric information*. And, of course, PRT lends itself to just such an analysis if one does not assume symmetric information at the renegotiation stage. It is this avenue that we pursue in this chapter. We introduce a buyer–seller contracting model with ex-post bargaining under one-sided asymmetric information based on Aghion, Fudenberg, Holden, Kunimoto & Tercieux (2012), and where the seller can make an ex-ante investment that increases the buyer's valuation, as in Che & Hausch (1999). This is also similar in spirit to Bester & Münster (2016), who emphasize the value of outside options in a closely related model of performance evaluation. We offer a model where only the presence of an outside option allows for approximately ex-ante efficiency. Without an outside option, any static or sequential mechanism performs worse, which we view as a rationale for the role of ownership allocation in contracting environments with asymmetric information. We take these theoretical predictions to a laboratory setting and find that outside options as implemented through asset ownership are valuable, not because of efficient ex-ante investment but because they reduce ex-post frictions.

Dr Johannes Hoelzemann,
Dept of Economics,
University of Toronto,
Toronto, ON M5S 2E9
CANADA

E-mail: j.hoelzemann@utoronto.ca

URL: https://www.unsworks.unsw.edu.au/primo-explore/fulldisplay?docid=unsworks_51696&context=L&vid=UNSWORKS&search_scope=unsworks_search_scope&tab=default_tab&lang=en_US

Journal & Proceedings of the Royal Society of New South Wales, vol. 151, part 2, 2018, p. 235. ISSN 0035-9173/18/020235-01

Thesis abstract

Doing laundry more sustainably: disrupting everyday practices through media conversations

Holly Kaye-Smith

Abstract of a thesis for a Doctorate of Philosophy submitted to Western Sydney University, Penrith, Australia

This research experiments with a video prototype created to activate user participation in ideas for washing clothes less. Washing clothes can be an extremely resource-intensive process, and only 7.5% of the clothes that we wash are considered 'heavily soiled,' suggesting many clothes are washed unnecessarily. Clothes laundering is referred to as a form of inconspicuous consumption because this routinised activity often goes unnoticed as it melds inconspicuously into everyday life. This is problematic because the routine of the activity can play a greater role in the clothes being washed than the actual need to clean the garments. This research recognises the significant environmental gains that could be met by reducing unnecessary habitual washing. A reduction in washing could be accomplished if clothes users simply questioned whether a garment was 'dirty' enough to be put through a washing machine. If the garment is not, the wearer may implement a less resource-intensive cleansing method, such as airing the garment on a hanger.

This research also responds to the need for social change advocates and people who make media, such as documentarians, film makers, designers and bloggers, people whom I refer to as 'media makers', to foster ground-up interventions and strategies for change that could help reduce consumption-related social and environmental problems. The research also explores how media makers can incorporate more inclusive approaches that involve their audiences in contributing to social change. The research seeks ways to equip clothes users with ideas and skills that bypass consumer-oriented ventures in order to capitalise on everyday activity as a form of activist intervention. The investigation began theoretically, and then moved on to auto-ethnographic trials that tested 'wash less' methods, before recruiting members of the public for conversational social research in a video shoot, five video screening and discussion groups, and finally laundry trials and interviews. The findings suggest that discussion about alternative laundering techniques can be harnessed as a form of disruption that can be augmented by practice-orientated media.

Dr Holly Kaye-Smith,
Digital Futures,
Western Sydney University,
Penrith NSW 2751
AUSTRALIA

E-mail: h.kaye-smith@westernsydney.edu.au

URL: https://researchdirect.westernsydney.edu.au/islandora/object/uws:44524/datastream/PDF/view

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Thesis abstract

Investigation of type I interferon and immune signalling in breast and ovarian cancer

Zoë R C Marks

Abstract of a thesis for a Doctorate of Philosophy submitted to Monash University, Melbourne, Australia

The type I interferons (IFN) are a family of innate immune cytokines known to play vital roles in host defence. The direct and indirect anti-tumour effects of these cytokines have led to considerable investigation into their role in cancer pathogenesis and their use as potential anti-cancer therapeutics. Despite this, the clinical use and benefit of type I IFN therapy has so far been limited to a select number of cancers such as melanoma and haematological malignancies. Notably, the success of IFN treatment has varied widely among patients and cancer types including many solid tumours where IFN therapy has exhibited poor efficacy and is largely restricted by dose-limited toxicity. The greater potential of these cytokines as anti-cancer agents has yet to be realised and to this end, there is a clear need to further understand the complexities of type I IFN signalling in cancer development and progression.

New insights into the molecular pathways underlying cancer progression reveal further evidence of dysregulated type I IFN signalling. Specifically, the presence of constitutive IFN signalling in mammary epithelium as well as primary breast tumours has been shown to be suppressed in bone metastases. Here, suppression of constitutive IFN was characterised as a critical mechanism of immune evasion facilitating successful breast cancer metastasis, although the processes underlying this metastatic pathway remained unclear. Meanwhile, a distinct type I IFN, IFNε, has been characterised as constitutively expressed in epithelial cells of the female reproductive tract (FRT), with previously unexplored anti-tumour properties, potentially critical in restricting FRT malignancies such as ovarian cancer. The significance of continuous IFN activity in the pathogenesis and additionally, the metastasis of these tumours remain to be characterised. The central aims of this thesis were to use these two models of cancer, breast and ovarian, to firstly: determine whether characterising IFN signatures in peripheral blood could provide further insight into cancer metastasis or derive novel biomarkers for patient stratification; and secondly: to investigate the previously unknown anti-tumour potential of a distinctly constitutive type I IFN, IFNε. In breast cancer, this work investigated local, systemic and distant signatures to characterise the processes underlying metastasis and map a continuum of disease progression from normal tissue to metastases. Blood transcriptomics revealed a strong enrichment of platelet activity, T cell suppression and broad IFN involvement, which were further investigated by multiplexed staining of tumour tissue to correlate key immune-tumour cell interactions with

metastatic potential. In ovarian cancer, this work demonstrated patterns of constitutive IFNε expression never before characterized – in the tissue of origin of high grade serous ovarian carcinomas (HGSC). In addition, this study demonstrated the first evidence of the loss of constitutive IFNε in human HGSC development and has also revealed IFNε to be an effective anti-metastatic therapy in mouse models of orthotopic and disseminated ovarian cancer, through both intrinsic and extrinsic pathways of tumour suppression providing the basis for the use of IFNε as an anti-cancer therapy.

Thus, this thesis contributes to the knowledge of constitutive type I IFN in tumorigenesis and tumour progression and demonstrates the potential use of endogenous IFN signalling and immune signatures for patient stratification in cancer progression as well as targeted anti-metastatic exogenous IFN therapy.

Dr Zoë Marks,
Medicine,
Monash University,
Clayton Victoria 3168
AUSTRALIA

E-mail: zoe.marks@monash.edu

URL: https://figshare.com/articles/Investigation_of_Type_I_Interferon_and_Immune_Signalling_in_Breast_and_Ovarian_Cancer/7122320

Journal & Proceedings of the Royal Society of New South Wales, vol. 151, part 2, 2018, pp. 238–239. ISSN 0035-9173/18/020238-02

Thesis abstract

Investigation into the biology of human Malignant Rhabdoid Tumour

Dean Popovski

Abstract of a thesis for a Doctorate of Philosophy submitted to Monash University, Melbourne, Australia

Malignant Rhabdoid Tumour (MRT) is a rare paediatric cancer of the kidney and CNS that is resistant to current treatment protocols. Prognostic outcomes remain poorest in infants under the age of one, yet frequency of MRT is highest in these patient cohorts. MRT is genetically characterised by homozygous inactivation of the *SMARCB1* gene, a critical subunit of the SWI/SNF chromatin-remodelling complex. Excluding *SMARCB1*, Next-Generation studies have revealed that there are limited additional recurrent genetic events present, implicating epigenetic deregulation in the pathogenesis of MRT. The ability of Histone deacetylase inhibitors (HDACi) to mimic the histone acetylation functions of the SWI/SNF complex in SMARCB1-null cells provides rationale for investigating the therapeutic potential of HDACi in MRT. Sustained treatment of human MRT cell lines with low-dose Panobinostat (LBH589) led to cell growth arrest and changes in cellular morphology. Transcriptional profiling of three independent MRT cell lines following 21-day treatment by Illumina Human HT-12 v4 Expression BeadChip revealed a marked increase in the induction of neural, renal, muscle and osteoblast differentiation pathways. Additionally, sustained low-dose LBH589 treatment *in vivo* resulted in tumour growth arrest associated with tumour calcification detectable by X-ray imaging. Histological analysis of LBH589-treated tumours revealed significant areas of ossification, confirmed by Alizarin Red staining. Immunohistochemical analysis demonstrated increased TUJ1 and PAX2 staining suggestive of neuronal and renal differentiation, respectively. These data suggest a SMARCB1-dependent SWI/SNF function important for lineage maturation. Re-expression of SMARCB1, in G401 MRT cells under a 4-hydroxytamoxifen inducible vector, phenocopied low-dose LBH589 treatment leading to cell growth inhibition, senescence and terminal differentiation *in vitro* and *in vivo*, suggesting mechanistic similarity. EZH2 is a core subunit of the transcriptional repressive complex, PRC2, which confers transcriptional silencing via the addition of methyl groups to Lysine 27 of Histone 3 (H3K27me^3). Coincidentally, EZH2 is a known HDAC recruiter and SMARCB1 transcriptional target, implicating EZH2 as a potential mechanistic candidate. Intriguingly, EZH2 expression and H3K27me^3 were drastically reduced following sustained low-dose LBH589 treatment in MRT cells. Corresponding reduction of EZH2 and H3K27me^3 was observed in G401 cells following re-expression of SMARCB1. Sustained siRNA knockdown of EZH2 in MRT cells resulted in reduced cell growth and cellular morphology changes associated

with differentiation and senescence. Q-PCR profiling revealed similar genetic signatures in EZH2 knockdown cells as those seen in low-dose sustained LBH589 and SMARCB1 re-expressed MRT cells. Unexpectedly, treatment of MRT cells with the EZH2-catalytic domain inhibitor, GSK126, had a moderate effect on EZH2 expression and partially reduced H3K27me^3 and cell growth at doses 1nM-10µM. Excitingly, MRT cells treated in combination with low-dose LBH589 and GSK126 demonstrated a greater reduction in cell growth, *in vitro* and *in vivo*, compared to single agent controls, revealing a synergistic relationship. These data suggest EZH2 is an important mediator of MRT proliferation and differentiation and provide evidence for dual therapeutic targeting of EZH2 with low-dose HDACi in MRT.

Dr Dean Popovski,
Faculty of Pharmacy & Pharmaceutical Sciences,
Monash University,
Parkville Victoria 3052
AUSTRALIA

E-mail: dean.popovski@monash.edu

URL: https://figshare.com/articles/Investigation_into_the_Biology_of_Human_Malignant_Rhabdoid_Tumour/5005385

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Proceedings of the Royal Society of New South Wales
The 2018 programme of events – Sydney

Held at the Union, Universities and Schools Club, 25 Bent St, Sydney; the State Library of NSW (SLNSW), Shakespeare Place, Sydney; Sydney Mechanic's School of Art (SMSA), 280 Pitt Street, Sydney or otherwise stated.

Thu 1 Feb	SMSA/RSNSW Enlightenment Series, lecture 3 at SMSA	Kim McKay, AO, Director of the Australian Museum	On learning, adaption and the enlightenment: the museum
Wed 7 Feb	1260th Ordinary Meeting 2017 RSNSW Scholarship winners at UUSC	Grace Causer, University of Wollongong You-wei Lin, University of Sydney Cara van dear Waal, University of Sydney - Australian Museum	Novel and artificial nanomaterials Pharmacological information with regards to life-threatening respiratory tract infections ' superbugs' Evolutionary history and diversity of mantis shrimps
Thu 1 March	SMSA/RSNSW Enlightenment Series, lecture 4 at SMSA	Paul Brunton OAM, State Library of NSW	On learning, adaption and the enlightenment: the library
Wed 7 March	1261st OGM and open lecture at UUSC	Leslie Burnett FRSN, Garvan Institute	Precision healthcare – the coming revolution in medicine
Wed 14 March	The Four Societies Lecture	Robin Grimes, Imperial College London	Exciting materials for energy applications in 2050

	Held in conjunction with the Nuclear Engineering Panel of the Sydney Branch of Engineers Australia, the Australian Nuclear Association and the Australian Institute of Energy. Held at University of NSW, Colombo theatre		
Wed 4 April	1262nd Ordinary Meeting and 151st Annual General Meeting at UUSC	Paul Fennell, Imperial College London	The decarbonisation of industry
Thu 5 April	SMSA/RSNSW Enlightenment Series, lecture 5 at SMSA	George Paxinos AO FRSN, UNSW	Global deflation: the enlightenment has failed
Wed 2 May	Pollock lecture	Andrea Morello FRSN, UNSW	Engineering for understanding: how building quantum devices unveils the meaning of quantum mechanics
	Held at Club Bar, Roundhouse, UNSW Sydney, Kensington 2052		
Fr 18 May	Annual Dinner: Distinguished Fellow's Lecture and presentation of the Society's 2017 awards at SLNSW	Guests of honour: The Society's Vice-Regal Patron, His Excellency General The Honourable David Hurley AC DSC (Ret'd), Governor of New South Wales and Tom Keneally AO DistFRSN	Mungo Man imagined: writing the ultimate historical novel
Wed 6 June	1263rd Ordinary Meeting at SLNSW	Ben Oldroyd FRSN, University of Sydney	No sex please. We're Cape bees.
Fr 22 June	RSNSW/SMSA Series 'Great Australians you have never heard of', lecture 1 at SMSA	Thomas Keneally AO DistFRSN	A Tasmanian convict who went from an Irish rebel to become Governor
Tu 26 June	ANSTO, AIP, RACI, RSNSW	Richard Garrett, ANSTO	Big science - exploring the future of the world's most exciting STEM challenges and development

	Held in conjunction with the Australian Institute of Physics, the Australian Nuclear Science and Technology Organisation, the Royal Australian Chemical Institute and RSNSW Held at the Discovery Centre, ANSTO, Lucas Heights		
Wed 4 July	1264th Ordinary Meeting at SLNSW	Joanna Mendelssohn FRSN, UNSW	Can art really make a difference?
Mo 23 July	RSNSW/SMSA Series 'Great Australians you have never heard of', lecture 2 at SMSA	Peter Baume AC DistFRSN	A scientist who chaired the group that eliminated a disease from the world
Wed 1 August	Poggendorff lecture at University of Sydney	Brent Kaiser, University of Sydney	Establishing a sustainable nitrogen diet to agricultural intensive cropping industries
Wed 8 Aug	1265th Ordinary Meeting at SLNSW	Muireann Irish FRSN, University of Sydney	The final frontier - on the complexity and frailty of human memory
	Sydney Science Festival science talks held at SMSA		
Mo 13 Aug 12.30-1.30	Sydney Science Festival	Ann Williamson FRSN, UNSW	Will self-driving cars make us safer?
Tu 14 Aug 12.30-1.30	Sydney Science Festival	Rosie Hicks, AustralianNational Fabrication Facility	Nanotechnology: what is special about small stuff?
Tu 14 Aug 6.30-7.30	Sydney Science Festival	Simeon Simoff FRSN, Wstern Sydney University	Ethics, emotions and elegance in artificial intelligence?
Fri 17 Aug 6.30-7.30	Sydney Science Festival	Phillip Norrie, FRSN	Wine and medicine: an Australian perspective
Wed 5 Sep	1266th Ordinary Meeting at SLNSW	Richard Kemp, University of NSW	Eyewitness evidence

Thu 6 Sep	RSNSW/SMSA Series 'Great Australians you have never heard of', lecture 3 at SMSA	Brynn Hibbert AM FRSN, UNSW	Three for the price of one: a day at the races
Wed 3 Oct	1267th Ordinary Meeting at SLNSW	Gordon Wallace AO FRSN, University of Wollongong	3D printing of body parts
Wed 7 Nov	1268th Ordinary Meeting at SLNSW	Tara Murphy, University of Sydney	Breakthrough! The detection of gravitational waves from a neutron star merger
Mo 12 Nov	RSNSW/SMSA Series 'Great Australians you have never heard of', lecture 4 at SMSA	Alison Bashford FRSN, UNSW	A geologist, geographer and anthropologist
Tu 13 Nov	2018 Postgraduate Awards Event	Tibor G Molnar, University of Sydney	Alice and Bob in Wonderland
	Held in conjunction with the Australian Institute of Physics, RSNSW and the Royal Australian Chemical Institute. Held at University of NSW, School of Physics, Old Main Building, level G		
Thu 29 Nov	RSNSW and Four Academies Forum	Government House, Sydney; hosted by his Excellency General The Honourable David Hurley AC DSC (Ret'd) Governor of NSW and Patron of the Royal Society of NSW	Towards a prosperous and sustainable Australia: what now for the lucky country?
	Held in cooperation with the Australian Academy of Science, the Australian Academy of Technological Sciences and Engineering, the Australian Academy of the Humanities and the Academy of Social Sciences in Australia. Held at Government House.		
Wed 5 Dec	1269th Ordinary Meeting followed by the Society's Christmas Party at SLNSW	Royal Society of NSW 2018 Jak Kelly Award: Anita Petzler, Macquarie University	Hydroxyl as a probe of the molecular interstellar medium

The 2018 programme of events—Southern Highlands

Held at the Mittagong RSL, 1st Floor, Joadja/Nattai Rooms.

Thu 15 Feb	Dr Madeleine Beekman University of Sydney, Life and Environmental Sciences	Are you smarter than a Slime Mould?
Thu 15 Mar	Dr Bradley Tucker Australian National University, Astronomy and Astrophysics	Black Holes and the Milky Way
Thu 12 Apr	Prof Anne Cutler University of Western Sydney, Speech and Language	Babies are working harder than we think
Thu 17 May	Prof Gordon Parker Scientia Professor of Psychiatry, University of NSW Executive Director of the Black Dog Institute	An Overview of Depression in our Society
Thu 21 June	Dr David Suggett University of Technology Sydney (UTS) Team leader, Future Reefs program	Future Reefs How climate change will impact coral reefs
Thu 19 July	Dr Ken McCracken Solar physicist, foundation director of CSIRO Office of Space Science and of CSIRO Division of Mineral Physics	Ice Ages, Big and Small
Thu 16 Aug	Dr Philip Cam President, Philosophy in Schools Association, NSW	Philosophy in Schools
Thu 20 Sep	Prof Bert Roberts University of Wollongong, Archaeological Science	When did Australia's History Begin?
Thu 18 Oct	Hugh Mackay Psychologist, Sociologist and Social Researcher	The State of the Nation starts in your Street
Thu 15 Nov	Dr Dana Cordell Research Director (Food Systems), Institute for Sustainable Futures, UTS (talk given by Dr Brent Jacobs, a colleague of Dr Dana Cordell).	Sydney Food Futures Competing priorities for Sydney's fertile farmland.

Awards 2018

1) James Cook Medal 2018 – Professor Elizabeth J. Elliott AM FAHMS

The James Cook Medal is awarded from time to time for outstanding contributions to both science and human welfare in and for the Southern Hemisphere.

Professor Elliott is an academic and a practising pediatrician who has dedicated her career to improving the health and quality of life, as well as human rights, of ill and disadvantaged children in Australia, the Asia-Pacific and beyond. Her translational research – laboratory, clinical and public health – has been at the forefront of advances in evidence-based pediatrics, rare diseases, gastroenterology, and fetal alcohol spectrum disorder.

Professor Elizabeth Jane Elliott AM FAHMS MD, MBBS, MPhil, FRACP, FRCPCH, FRCP is a Distinguished Professor of Paediatrics and Child Health, University of Sydney and NHMRC Practitioner Fellow, Children's Hospital, Westmead. Prof Elliott received her MBBS (1980) and MD (1992) from the University of Sydney. From 1983 to 1991, she held appointments at major hospitals in London and Leicester. Since 1991, she has been the University Sydney, where she was promoted to Professor in 2006.

2) Edgeworth David Medal 2018 – Associate Professor Elizabeth J. New

The **Edgeworth David Medal** is awarded each year for distinguished research by a young scientist under the age of 35 years for work done mainly in Australia or for contributing to the advancement of Australian science.

Associate Professor New is an inorganic chemist who has contributed to our understanding of the body's internal environment by developing new fluorescent probes for sensing key cellular events, including oxidative stress, metabolism of anticancer agents, and intracellular metals. She has already built a productive group of researchers in her laboratory as well as developing collaborations with 40 other laboratories in Australia and around the world.

Associate Professor Elizabeth J. New FRACI FRNS is a member of the School of Chemistry, the University of Sydney. She earned her BSC (Hons 1 and Medal, 2005) and a MSc (2007) from the University of Sydney. She was awarded her PhD in Chemistry (2010) from Durham University (UK). She was a postdoctoral fellow at the University of California at Berkeley (2010-2011) before returning to the University of Sydney as an ARC Discovery Early Career Researcher Award Fellow. She was promoted to Associate Professor in 2018.

3) Clarke Medal for Zoology 2018- Professor Emma Johnston AO FRSN

The **Clarke Medal** is awarded each year for distinguished research in the natural sciences conducted in the Australian Commonwealth and its territories. The fields of botany, geology, and zoology are considered in rotation. For 2018, the medal was awarded in Zoology.

Professor Johnston has made exceptional contributions to our understanding of community ecology of marine animals, especially the human activities as drivers of ecosystem dynamics –

human activities. Her research that not only advances our fundamental knowledge in ecology, but also directly enables significant improvement in the management of marine systems.

Professor Emma L. Johnston AO FRSN is Dean of Science at the University of New South Wales, Sydney. She received her BSc (Hons, 1997) and PhD in Marine Ecology (2002) from the University of Melbourne. In between those degrees, she also earned a Certificate III in Mandarin, RMIT TAFE (2001). She has held academic appointments in UNSW's School of Biological, Earth and Environmental Sciences since 2001. She was promoted to professor in 2014, and became Dean of Science in 2017. She has also been in demand for external roles, including Director Sydney Harbour Research Program (2012-2015) and Board Member, Great Barrier Reef Marine Park Authority (2016-ongoing).

4) History and Philosophy of Science Medal 2018 – Professor Paul E. Griffiths

The Society's History and Philosophy of Science Medal is awarded each year for outstanding achievement in the History and Philosophy of Science, especially the study of ideas, institutions, and individuals of significance to the practice of the natural sciences in Australia.

Professor Griffiths has brought philosophical, biological, cognitive science, and biomedical researchers into genuine dialogue. In his recent work in the philosophy of medicine, he has challenged received ideas about health and disease, producing a novel model that accommodates recent transformational discoveries such as the 'developmental origins of health and disease' and the role of the microbiome.

Professor Paul E. Griffiths FAAH, FAAAS, FRSN is a Professor of Philosophy, University of Sydney, where he heads the Theory and Method in Bioscience project node of the Charles Perkins Centre, which conducts multidisciplinary research in global health. He received his BA (Hons, 1984) from the University of Cambridge and his PhD (1989) from Australian National University. From 1988 to 1998, he was located at the University of Otago. From there, he progressed to the University of Sydney (1998-2000), the University of Pittsburgh (2000-2004), University of Queensland (2004-2007), and back to the University of Sydney (2007 to present), plus a visiting professorship at the University of Exeter (2007-2016).

5) Poggendorf Lecture Award 2018 - Professor Robert F. Park

The Poggendorf Lecture is awarded every two to three years for research in plant biology and more broadly agriculture.

Professor Park is a world leader in understanding the genetic and molecular interactions of plants with fungal pathogens, most notably plant resistance to cereal rust. His research has made significant, beneficial contributions to international efforts to control these diseases at a commercial scale.

Professor Robert F. Park FAATSE is located at The University of Sydney's Plant Breeding Institute. He earned his BSc (Hons, 1980) and his PhD (1984) at La Trobe University. His first appointment was with the Queensland Department of Primary Industries, and in 1988, he was appointed as a Research Fellow at the University of Sydney.

6) Jak Kelly Award

The winner of the **Jak Kelly Award** for 2018 is **Anita Petzler** from Macquarie University. Her research investigates the use of hydroxyl as a tracer of diffuse molecular gas to complete our understanding of the mechanisms of star formation.

The Jak Kelly Award encourages excellence in postgraduate research in physics. The winner was selected from a short list of candidates who made presentations at a recent joint meeting at UNSW of the Australian Institute of Physics NSW Branch, the Royal Australian Chemical Institute, and the Royal Society of NSW.

7) Royal Society of NSW Scholarship 2018 – Ms. Evelyn Todd

The Royal Society of NSW Scholarships are awarded annually to research students in a university in either NSW or the ACT. A maximum of three scholarships of $500 and a complimentary year of membership of the Society are awarded in order to acknowledge outstanding achievement in any field of science.

Ms. Todd is PhD candidate the University of Sydney writing a thesis entitled "Inbreeding and Performance Genetics in Horses." She has recently published a scientific paper concerning genetic trends in thoroughbred horses over the past 300 years. This paper will be the first chapter of her thesis, which has already attracted interest in the horseracing industry. She worked in the training, management, and care of thoroughbreds while earning her BSc (Hons, 2015) at the University of Sydney.

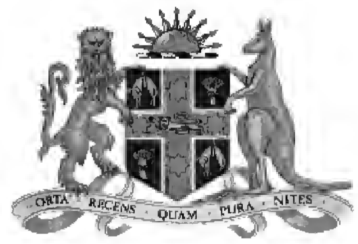
8) Royal Society of NSW Scholarship 2018 – Ms. Fiona McDougall

The Royal Society of NSW Scholarships are awarded annually to research students in a university in either NSW or the ACT. A maximum of three scholarships of $500 and a complimentary year of membership of the Society are awarded in order to acknowledge outstanding achievement in any field of science.

Ms. Fiona McDougall is a PhD candidate at Macquarie University writing a thesis aimed at understanding the possible transmission of pathogenic bacteria between humans and grey-headed flying foxes, which are a threatened species. Of special interest are the risks of spreading antibiotic-resistant bacteria in both populations. The results are intended to inform conservation and management strategies for the bats. Ms. McDougall is a registered veterinarian, having earned her BVS (1998) from the University of Sydney and Master of Veterinary Studies in Conservation Medicine (2013) from Murdoch University. She has extensive experience in biomedical and wildlife research.

Note on Gazetting

The Government Gazette of the State of New South Wales is managed by the New South Wales Parliamentary Counsel's Office and has published Government notices, regulations, forms and orders since 1832. It went on line in 2001 and since 2014 is only to be found at https://www.legislation.nsw.gov.au/#/gazettes .



Government Gazette

of the State of

New South Wales

Number 13

Tuesday, 6 February 2018

On the initiative of RSNSW Fellow Robert Whittaker AM FRSN the Society approached His Excellency the Governor to formally gazette fellows of the Society. All current fellows were included in the first gazetting in 2018, and subsequently at the beginning of each year fellows elected in the previous year will appear in the Gazette.

As the Gazette of Tuesday 6 February 2018 says:

> "His Excellency, General The Honourable David Hurley AC DSC (Ret'd), Governor of New South Wales, as Patron of The Royal Society of New South Wales and in furtherance of the aims of the Society in encouraging and rewarding the study and practice of Science, Art, Literature and Philosophy, is pleased to advise and acknowledge the election of the following as Fellows and Distinguished Fellows of the Society."

Fellows

Proven leaders and experts in their field, entitled to use the post nominal FRSN. Please note Professorial titles – including adjuncts, conjoint, and professors of practice – have been used where applicable. Details as to their field of expertise, their resident university (or universities) or institution may be ascertained from the Royal Society of New South Wales.

ADAM, Emeritus Professor Christopher Adam FRSN

AGGLETON, Scientia Professor Peter Aggleton FRSN

AITKEN, Laureate Professor John Aitken FRSN

ANDERSON, Professor Lyndon Anderson FRSN

ANDERSON, Professor Warwick Anderson FRSN

ASLAKSEN, Dr Erik Aslaksen FRSN

BAILEY, Professor Ian Bailey AM FRSN SC

BALDRY, Professor Eileen Baldry FRSN

BANATI, Professor Richard Banati FRSN

BARLOW-STEWART, Associate Professor Kristine Barlow-Stewart FRSN

BARNETT, Professor Allen Barnett FRSN

BASHFORD, Professor Alison Bashford FRSN

BEAUMONT, Professor Emerita Joan Beaumont FRSN

BEEKMAN, Professor Madelaine Beekman FRSN

BELL, Dr Graham Bell FRSN

BERTRAM, Hon Associate Professor Christopher Bertram FRSN

BLACK, Professor David Black AO FRSN

BLACKWELL, Ms Cecelia Blackwell FRSN

BLAXLAND, Professor Colonel John Charles Blaxland FRSN

BONTHORNE, Mr Ross Bonthorne AM FRSN

BRANAGAN, Associate Professor David Branagan FRSN

BRIGGS, Dr Barbara Briggs FRSN

BROADFOOT, Professor Alan Robert Broadfoot FRSN

BRUNGS, Vice Chancellor Professor Attila Brungs FRSN

BURFORD, Professor Robert Burford FRSN

BURNETT, Professor Leslie Burnett FRSN

BURTON, Professor Michael Burton FRSN

BÜTTNER, Dr Herma Gertrud Büttner FRSN

CAMPBELL, Grahame Campbell FRSN

CARMODY, Dr John Carmody FRSN

CASTLES, Distinguished Professor Anne Castles FRSN

CATERSON, Professor Ian Caterson AM FRSN

CHRISTIAN, Professor David Christian FRSN

CHUBB, Professor Ian Chubb AC FRSN

CLANCY, Professor Emeritus Robert Clancy AM FRSN

COHEN, Associate Professor David Cohen FRSN

COLLINS, Dr Michael Collins PSM FRSN

CONIGRAVE, Professor Arthur Conigrave FRSN

COOK, Dr David Cook FRSN

COPLEY, Mr Gregory Copley AM FRSN

CORKISH, Dr Richard Corkish FRSN

COSTER, Professor Emeritus Hans Coster FRSN

CRAIN, Distinguished Professor Stephen Crain FRSN

CROSSLEY, Professor Maxwell Crossley FRSN

DAIN, Professor Emeritus Stephen Dain FRSN

DANIELMEYER, Mr Hans Danielmeyer FRSN

DANOS, Mr Trevor Danos AM FRSN

DAVIS, Dr Jessica Milner Davis FRSN

DAWES, Professor Emeritus Ian Dawes FRSN

DAWSON, Professor Kenneth Dawson FRSN

DEMUTH, Distinguished Professor Katherine Demuth FRSN

DIXON, Distinguished Professor Nicholas Dixon FRSN

DODDS, Professor Susan Dodds FRSN

DUNN, Professor Kevin Dunn FRSN

DZURAK, Scientia Professor Andrew Dzurak FRSN

EGGLETON, Professor Benjamin Eggleton FRSN

EINAV, Professor Itai Einav FRSN

ELDER, Dr John Richard Elder AM FRSN

ENFIELD, Professor Nicholas James Enfield FRSN

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The Royal Society of New South Wales
P.O. Box 576
Crows Nest, NSW 1585, Australia

info@royalsoc.org.au (general)
editor@royalsoc.org.au (editorial)
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